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EINSTEIN'S PRINCIPLE OF RELATIVITY AND ITS BEARING UPON PHYSICS.*

BY

JOSEPH S. AMES, Ph.D., LL.D.

Director of Physical Laboratory, The Johns Hopkins University, Baltimore, Md.

WHEN I received the invitation to address The Franklin Institute on the subject of Einstein's Theory of Relativity, I hesitated for many days before accepting it; and even now, with my thoughts in order and my words in mind, I am not convinced that I can accomplish what both you and I are hoping I can do: make the work of Einstein clear to a group of people who are not specifically physicists or mathematicians. If you were the latter, that is, if you had devoted yourself recently for a number of years to an intensive study of physics and mathematics, I am certain that I could explain to you the main features of Einstein's work—by at least a month of class lectures. But what can I do in one hour? The best I can really hope to do is to explain the main outline of Einstein's contribution to our philosophy, emphasizing only a few, very few, points.

The underyling principles of Einstein's theory are exceedingly simple; *they* may be made clear; but the way in which they are used is not as simple; to understand this requires not alone

* Presented at the Stated Meeting of the Institute held Wednesday, October 20, 1920.

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an acquaintance with many physical phenomena but also an appreciation, at least, of the power of mathematics. I must either take this as granted or ask you to believe me.

The purpose of a lecture such as this is not, I am assuming, to convince you that Einstein is right in his method and conclusions, nor to carry you step by step through his reasoning, but, rather, to state the problems he had in mind and to indicate how he attacked them. At the very beginning I must state two facts which may not be known to you all. The first is that, although the deduction of a new law of gravitation by Einstein has appealed to the imagination of the public, this is only an incidental feature in Einstein's work; other consequences are really of much greater importance, even to the world at large. The second is that it is not correct to say that Einstein has proposed a "theory" of anything, in the sense of giving a picture of any mechanism. We speak properly of the "corpuscular" theory of light, a theory advanced by Sir Isaac Newton that light as a sensation is due to streams of minute particles which leave the luminous body and finally, after passing through space or portions of matter, enter the eye and affect the retina. Or we may speak of the "kinetic" theory of matter, meaning by that that we try to account for many of the properties of matter by attributing to its minute moving parts, its atoms and molecules, the same characteristics which we find by experience are common to large portions of matter, such as billiard balls or croquet balls. Thus, in using the word "theory" of any phenomenon we ordinarily mean that we are proposing a picture or description of it in terms of more familiar or more simple facts. Now Einstein does not in any sense give us a picture of gravitation, nor does he attempt to describe it in terms of other phenomena. As a matter of fact, in the course of the development of his concept of nature, he finds a mathematical statement of the facts of gravitation as observed by us.

Let us see what these facts are, and incidentally discuss the method used by Newton in his great Principia. Newton and Galileo before him studied with the greatest care the simplest cases of motion and matter, such as the impact of one body against another, a body falling towards the earth or thrown up away from the earth, an arrow or bolt shot from a cross-bow, etc. Both these great philosophers sought to express their observations in simple form, and to formulate certain laws or postulates,

which, if true, would account for their observations. In this connection Newton found it convenient to introduce the use of the word "force" and give it a precise meaning, the line of thought being as follows: He and Galileo both expressed their first law of motion by saying that, if a material body is in motion and is far removed from all other bodies, its state of motion will not change. Naturally, this cannot be proved by any experiment or observation of ours; it is a question of belief, a portion of a creed. Then, if we observe that the motion of a body is changing, we use the expression, following Newton, "there is a force acting on" the body. This is, so far, nothing but a form of words, a description of the fact that the motion is changing. Thus, if we see an apple falling from a tree, we note that, as it falls, it goes faster and faster, its motion is changing each instant; we say there is a force acting on it, and speak of the "force of gravity." We are not attributing any property to the apple or to the earth; we are simply using a special word to describe an observed fact. Or again, if we tie a stone to one end of a string and, holding the other end in our hands, make the stone whirl in a circle, we see that the motion of the stone is changing each instant, its direction is altered continuously; we say that there is a force acting on it, the force of the tension of the string. Similarly, when the motion of the moon is considered, it is observed to be in a nearly circular path around the earth, its direction is changing continuously, here again there is the action of a force. Newton proceeded further, and in terms of the mass of the body, that is its quantity of matter, defined the force exactly, giving it a definite mathematical value. To Newton it was evident that the force acting on the falling apple and that acting on the moon were both illustrations of a common phenomenon; both were associated with the earth. He propounded the idea that between any two material bodies in the Universe there was a force acting, that is, if they were first held at rest and then let go, they would approach one another with a greater and greater speed. As a matter of fact, the earth does rise so as to meet the falling apple and the moon is continuously falling in towards the earth, but only so far as to keep in its circular orbit. This is Newton's principle of Universal Gravitation, or at least part of it. The other part, that of stating in mathematical language the law of gravitation immediately engaged Newton's

attention. He required as a starting point exact knowledge of the facts of observation; that is, of the motion of the falling apple, of the moon and of the planets in the solar system, for evidently these last had their motion subject to this desired law of gravitation. He needed to know how rapidly the speed of the apple changed, whether other falling bodies of different substances and different weights fell in exactly the same way an apple did; and here a knowledge of Galileo's famous experiments on the fall of different bodies from the top of the Leaning Tower of Pisa and on pendulums having different bobs helped him, indicating that so far as gravity alone was concerned all bodies, of varying sizes and material, had the same motion as they fell to the earth, that is, the acceleration of their velocities was the same. He also wished to know the size of the moon's orbit and its period of revolution, the nature of the orbits of the planets, how rapidly they moved and their periods of revolution around the sun. Fortunately he had at his disposal the knowledge required. So far as the planets were concerned Kepler had discovered by many years' patient study of Tycho Brahe's astronomical observations certain facts, known as Kepler's laws. Newton then tried to imagine some simple statement applicable to the Universe such that, if it were true, the known facts of observation could be deduced by mathematical processes. He succeeded in proving that, if there is a force of attraction between two particles of matter of masses m_1 and m_2 at a distance of r apart which is proportional to the product of their masses and inversely to the square of their distance, *i.e.*, to $m_1 m_2/r^2$, then all the observed phenomena so far as known could be deduced. This is Newton's law of universal gravitation: it is not a theory, there is no correlation with other natural phenomena, it is simply an attempt, and a most successful one, to express a countless number of facts in a simple form. The experience of over one hundred years, all the predictions of astronomy so amply verified, have confirmed our belief in the accuracy of Newton's law. There is, however, one feature of universal gravitation about which there was no knowledge in Newton's day nor even in our own till most recently. This is a question which might very easily be asked; is the law of gravitation different for bodies moving slowly and those moving rapidly? The greatest velocity known is that of light, about 186,000 miles per second; and in comparison with this the veloci-

ties of the planets may in general be called small. So it is perfectly possible that Newton's law is a statement of fact for bodies moving slowly, but requires modification in case they are moving rapidly. Such now appears to be the case; we have knowledge of the action of gravitation in the case of great velocities; and Newton's law does not express the facts. Moreover, Einstein has proposed a law which does correspond in a wonderful manner with these and all known facts. Two points in this connection may well be emphasized. Einstein has not disproved Newton's law, he has simply described the limitations within which it holds. Of course, if any one were to claim that Newton's law tells the whole truth about gravitation, then it would be fair to say that Einstein had disproved this claim. Again, Einstein did not obtain his law by trying to discover a mathematical form for known facts, nor did he try to make slight modifications in Newton's law so as to make it fit better with new facts. What he did was to take a general view of all natural phenomena, as revealed to us by our observations and measurements, and then to formulate certain general principles which appealed to his philosophy, principles so simple that they admitted of proof or disproof only by drawing conclusions from them and comparing them with direct observations. Among these deductions was a new statement of a law of gravitation. It cannot be expressed in a simple manner, unless we make certain limitations upon our conditions. In particular, if we wish to apply the law to cases where the velocity is small, it takes the form proposed by Newton.

I do not believe that any one here is interested in knowing the exact statement of Einstein's law nor in hearing details of the facts of which it is the expression. What every one wishes to know is the method followed by Einstein: what were the general principles of nature proposed by him, how did he come to make them and how did he draw conclusions from them. I shall do my best to explain these matters to you, and I hope I can make you share in the wonder and admiration I feel in thinking of the genius and ability so clearly shown by Einstein in all his work.

When we consider any phenomenon of nature, for instance, a falling body, we realize that before we can state any facts about it—facts of importance—we must be able to measure, or give numbers to, the various quantities concerned. We must know how fast it is moving at any instant and how much this velocity

changes during a certain definite interval of fall; or else we can start by making some assumption in regard to the motion and, after deducing the consequences and comparing them with observations, decide whether our assumption is right or wrong. In either case we need to be able to give numbers to the quantities observed. Two fundamental quantities are intervals of length and intervals of time. Every one knows how we measure them in practice. To measure a length, for instance, that of a table, we select first of all a measuring rod, which, as a rule, is divided and marked with divisions equally spaced, and then by applying the rod along the edge of the table, marking its ends, replacing it, etc., we finally can obtain the length desired. If living in America or England, we would use ordinarily—not in a scientific laboratory—a yard-stick subdivided into fractions of an inch; if we were working in a laboratory or were living anywhere else in the world, we would use a metre-rod subdivided in centimetres and millimetres. The method used would be the same. This is called the method of superposition, and it involves the definition of what we mean when we use the words "two lengths are equal," namely, we say two lengths are the same if the two intervals of which we are speaking can be superimposed. In other words, to measure a length we adopt some unit and find how many times it is contained in the length. But when we consider an interval of time and the means we use in giving it a number, we recognize at once that a different method is required. To propose the selection of a unit of time and to see how many times it is contained in the interval in question is to talk nonsense; we cannot put our hands on a unit of time, and we have nothing to compare it with, for when time passes it is gone forever. What we actually do, as you all know, is to count the seconds or minutes elapsing in any time interval by looking at a watch or a clock. This practice is based upon our belief that the time intervals between two ticks or beats of a clock are the same. Of course, we can never *prove* that this is true; for if I were to maintain that the pendulum of a clock did not swing regularly, how could you disprove my assertion? Certainly not by comparing it with another pendulum, because I might say that it also was not swinging regularly. As a matter of fact, our method involves the definition of what we mean by the words "equal intervals" of time. It is a perfectly good method, as a hundred years of observation have shown. In

this connection I must also call to your attention the idea involved in saying that two events are simultaneous. Suppose, for instance, two men a mile apart raise their right hands, how do I decide if they do it simultaneously? I might stand near one of them, in some position so that I can see both, and might then watch them carefully. But it takes time for light to come from the distant man to my eye; and so, when they are apparently raising their hands simultaneously, they are not; the man at a distance is slightly ahead of the other. This method can be remedied if I station myself half way between the two men and then by means of mirrors arrange so that I can watch both men at the same time. Then I can decide if their motions are simultaneous. This conviction of mine depends upon my belief that the velocity of light is the same in both directions and that it is a constant unchanging quantity. Einstein, in substance, proposes as a method of defining equal intervals of time, not the selection of pendulum swings, but the use of light signals. His definition of equal intervals is the times required for light to traverse equal lengths. This is really a better definition.

We are now ready for the first step in Einstein's process of thought. In discussing, so far, lengths and intervals of time we, as a rule, think of the length to be measured and the clock at which we are looking as being at rest with reference to ourselves. The question may not be so simple if they are moving. For instance, if a railway train is in motion, how would we measure the length of one of its cars? We would station several observers along the tracks; then, as the train passed, two of these might be so placed that they could note *simultaneously* the two ends of the car by making marks on the ground near the track; and later by means of our measuring rod we would measure the distance between the two marks. Another observer travelling on the train and having a rod the same as the others are using on the ground could also measure the length of the car. Would the two measurements give the same value for the length? Einstein raised this question and showed that, before we could answer it, it was necessary to be very definite as to what we meant by our words and as to what we believed in regard to observations made of stationary and of moving objects. One of the simplest cases familiar to us of observations of moving bodies is furnished by two trains moving on parallel tracks. To begin with, if a person

were in a train moving at a uniform rate, he would be entirely unconscious of the motion if he didn't look out of the window; for the only thing that in practice makes a traveller know that he is moving is the almost constant occurrence of slight irregularities in the motion and certain sounds. Now, if our two trains are moving uniformly, but one more rapidly than the other, when one passes the other, an observer in one train looking out of the window at the other cannot decide whether he is at rest and the other train moving, or he is moving and the other train is at rest, or both of them are moving. How often has it happened to each one of us, when we have been sitting in a train in a station watching another train on the next track, that, if this train slowly moved past the window, we were uncertain as to which train was actually moving, our own or the other? When one train moves by another, there is what is called relative motion between them; and every one is familiar with many illustrations of such motion. An observer on either body, so long as the relative motion is uniform, not jerky, cannot, by simply looking at the other, decide anything as to its own motion; he may be at rest, he may be moving slowly or rapidly. All he knows is that there is *relative* motion. In regard to absolute motion, *i.e.*, his own motion, he knows nothing. Einstein generalized this visual experience of ours by stating, in the form of what mathematicians or logicians call a "postulate," that in the case of uniform relative motion, such as our two trains, it is impossible for an observer moving with either body to decide about his own motion by any means at all, he may look at the other body, or he may try any means, using sound, magnetism, or electricity. Expressed slightly differently this says that if two observers are attached to two systems which have uniform relative velocity the phenomena and laws of nature will be the same for them both. This postulate appears to every one, I am sure, as perfectly reasonable and in accord with his own experiences. It is one of those statements which cannot be proved directly, however; it is too general. We must draw as many conclusions from it as possible and see if they are confirmed by experience.

If an observer on a moving train wished to make measurements of time, or to decide whether certain events are simultaneous, he would use light signals. But will the velocity of light as observed by him be the same as it would be if the train were at

rest? Here we must not fall into a logical trap. We have agreed, following Einstein, to use light in our method of defining intervals of time; so we shall have no way of answering the question directly. But indirectly there is evidence that it is the same for an observer at rest or on the moving train. Of course, the observer whom we speak of as moving might say that he was at rest and we were moving. All that is known to him or to us is that there is relative motion. So Einstein makes as his second postulate; the velocity of light, no matter how observed, or what its source, is always the same. Of course, in this he means, as he clearly says, the velocity of light in a vacuum; but the presence of the air makes no difference requiring attention. (It must be added that this postulate requires modification when we deal with an intense gravitational field.)

These two postulates may appear to you very simple and self-evident; almost of trivial importance. But their consequences are by no means so. They may, in fact, be fairly called revolutionary to many of our preconceived ideas.

It is now a simple matter to answer the question which was asked some time ago, that concerning the length of a railway car as measured by a man in the car or by observers along the track. It may be proved that to the latter the length is slightly less than to the former. This difference in length is extremely minute, too small for measurement in the case of any railway train, but it is definite. If there is a yard stick lying lengthwise in a car of each of our two trains which are in relative motion, either observer looking first at his own stick and then at the other would say that the one which appears to him to be moving looks shorter than the other. So the length of a rod has no absolute meaning, it all depends upon whether the man looking at it or measuring it is at rest with reference to it or not. If, however, the yard sticks are lying across the cars or are standing on end, the appearance of either rod is the same to both observers. Thus a body which appeared spherical to one would look like an ellipsoid to the other. You note that I do not say that the length or shape is actually changed; all I am saying is that bodies look differently and give different measurements to people who are at rest with reference to them and to people who are not. I must not speak of absolute or real rest, or motion, or lengths, but only of facts of observation.

Another question in regard to systems having relative motion

can also now be answered. Suppose there is a pendulum clock on one train and suppose there is some movement going on, for instance, suppose a man is writing or working a typewriter, how will they appear to a man on the other train? It is proved easily that the pendulum will appear to swing more slowly, and the man who is writing will apparently be working at his task more leisurely than would be the case if the observer were on the first train. A general statement is that if there is relative motion between two systems, all bodies in one system will appear contracted in the direction of motion to an observer in the other system, when he compares his impressions with those he has on looking at similar bodies in his own system; and all time intervals in one system appear to an observer in the other to be prolonged. It follows, too, that two events which appear to one observer to be simultaneous would not appear so to the other. Therefore, one sees how careful one must be in describing things. We have, thus, another illustration of the same object appearing differently to different people, the case of the two sides of the shield. If we had here on the earth a large body which to a man on the earth appeared spherical, and, if a man were flying over the earth in an imaginary airplane at the imaginary speed of 150,000 miles per second, he would say, in looking at the body, that it was like an egg standing on its end. If he were to see men walking about he would wonder how they could walk so slowly and not fall over. The really important fact to note is, as said before, that we cannot speak of the absolute or real length of a body, or the real value of an interval of time. The words have no meaning. Whenever any measurement is given we must always state something about the observer: is he moving with the thing measured, or is he not?

I could fill all the rest of my hour in telling you of the many striking consequences of a realization of this fact. Einstein called attention to many of them, but they are of interest in the main to physicists only. It is important to add that every deduction so far made which is capable of experimental investigation has been verified.

As I have been careful to note, Einstein's early work referred to bodies whose relative motion is uniform. Now in nature nothing is rarer than uniform motion; all the motions we have ever observed have been varying, or at the best have been uniform

for only short periods. Therefore, it was very natural for Einstein to consider if there were any general statements which could be made in regard to two systems whose relative motion was non-uniform. He did convince himself that there were such statements, and these additional postulates form the basis of many deductions, among them the law of gravitation. Before stating these postulates, however, I must take a few minutes for the explanation of some simple mathematical methods. Einstein made the perfectly obvious, but most important, remark that all of our knowledge of nature is in reality nothing but the perception of a series of coincidences. If I say that the temperature of this room is 70° F., what I mean is that I have a Fahrenheit thermometer, and on looking at it I observe that the end of the mercury column coincides with the 70th degree division. If I study a falling body, all my knowledge comes from rays of light coinciding with the retina of my eye. Therefore, all the laws of nature which we have deduced from our experiences, or which we may deduce in the future, must be statements concerning coincidences. To describe in mathematical language any event requires an expression of two things: first, the location of the event in space, *i.e.*, where it takes place, and secondly, the instant of time when it occurs. We have an extremely simple method of doing both of these. Suppose I wished to describe to some one the exact position of the corner of this desk with reference to this hall. I could measure its distance above the floor and its distances from two of the walls meeting in a corner; a knowledge of these three lengths definitely locates the corner. The process you see is this. I select three planes—the floor and two walls in this case—which are mutually perpendicular, and I measure the distances of the point in question from these three planes. These lengths are called the space coördinates of the point with reference to the three planes, or with reference to the three lines, or axes, in which these planes intersect. In this manner, using any reference system we wish to select, we can describe the location of any event. Naturally, if I use a different reference system, I shall have different coördinates. To describe the instant of time requires simply the adoption of some standard clock. The reading of this will give us the time coördinate of the event. The description of the event requires, therefore, the determination of four coördinates. If we are observing any phenomenon, for in-

stance a falling body, it is clear, I suppose, to all that the description of the motion will depend upon the reference system of coördinate planes or axes adopted. Thus, if we choose our axes fixed on the earth, one vertical, one North and South, and the third East and West, we would describe the motion as parallel to the vertical axis. Suppose, however, we are in a moving railway car and choose our axes as follows: one perpendicular to the floor, one along the car and one across it; then, if through the window we observed the falling body, for instance a raindrop, we would describe its motion as being oblique, not vertical; it is not parallel to the edge of the window. Again, suppose we are in an airplane and choose our axes fixed in it, then, if while the machine is manœuvring we observe a falling raindrop, we would say that it had an apparently very complicated motion. Thus our description of any phenomenon will vary with the set of reference axes adopted. But suppose that two events coincide, then it is evident that the statement of this fact is quite independent of our selection of axes. For instance, if a raindrop hits the umbrella of a man who is walking, it is true that the description of the path of the drop as it is falling and of the man while he walks will depend upon whether we are at rest on the earth, or in the moving train or in the airplane; but the statement of the coincidence of the path of the drop and that of the man's umbrella is the same in all cases. Thus the description of a coincidence is independent of our selection of reference axes. We immediately see the importance of Einstein's remark about the laws of nature being simply a formulation of facts about coincidence of events. Since this is the case, the form of any law of nature must be the same for all systems of reference axes. This, as a matter of fact, is Einstein's postulate, what he calls the "Principle of General Relativity."

Among the laws of nature is to be sought the law of gravitation. If we describe the motion of a falling body or of a planet, our description will, as I have explained, depend upon our choice of reference axes; but when we come to generalize our observations and express them in a law—as Newton attempted to do—we can be perfectly sure, in accordance with this principle formulated by Einstein, that its form will be the same no matter what reference system we adopt in order to state the mathematical facts. This statement is applicable to all other laws of nature,

not simply to a law for gravitation. We are thus introduced to a mathematical problem, namely the investigation of those mathematical expressions which keep the same form when we use different systems of coördinates. This is a well-developed branch of mathematics, but one which up to the past few years, till in fact Einstein showed its importance in the study of natural phenomena, was comparatively unknown to most students of physics. Recently, however, we have all been obliged to go back to college days and devote strenuous weeks to mathematical study.

Let us now see if we can follow Einstein's mode of thought in investigating gravitation. He knew, of course, that the law stating the facts must have the mathematical properties just described; he knew also that the true law must have such a form that under ordinary conditions it reduced to Newton's because this last expresses most of the known facts so well; but all this didn't help much. He wished to find some general method of describing and comparing phenomena in a space or region where there is no gravitational action with phenomena in a region where there is this action, that is, in what we call a gravitational field.

It is evident that if we were to perform any mechanical experiments near the earth or if they were made near the sun or near the moon, there would be complications owing to gravity; but if we were to be far off in space, away from the solar system, gravitation, as we know it here, would not trouble us. So if we had a space with its reference system of axes attached, as it were, to the "fixed" stars, it would evidently be the simplest one for us to consider. This is called a Galilean space or system. In such a space, since there are no gravitational effects, a body would not "fall" in any direction; if at rest, it would stay at rest without any support; if thrown by anyone, it would move in a straight line. I mean by this that these would be the facts observed by a man who was at rest in the system. Whereas in a gravitational field, that is, in a region where gravity is felt, if a body is let go from rest, an observer at rest will note that it will fall in the direction of the field; if it is thrown in any direction across the field, he will say that it has a curved path, like that of a stone which a boy on the earth throws up in the air. Einstein remarked that we could also secure, if we wished to, a Galilean space upon the earth, or the sun, or anywhere, by a very simple device. Suppose we had a large box, as large as a room, so that

an observer might stand inside and perform different experiments, and let this box be dropped from some height so as to fall freely towards the earth. The man inside, if he couldn't see out, would be entirely unconscious of the fact that he was falling and, in fact, of any gravitational sensation. He would not feel himself pulled down, nor, as a matter of fact, do we here on earth. We are conscious rather of being pushed up by the floor on which we are walking; but the man in the box does not have this sensation. So far as he is concerned gravitation does not exist. If he is holding a stone in his hand and simply releases it, it will not fall so far as he knows, it will appear to stay where it is; if he throws the stone in any direction, its path, as he observes it, will be a straight line. So the space in the box is a Galilean one. But if there is a man on the earth, or on a building, who can look into the box, he will say that the stone released from the hand is falling and that the path of the stone when it is thrown is curved; in other words, he will describe gravitational phenomena. Thus what Einstein had to do was to describe the connection between the description and measurements of phenomena going on in the box as made first by the man inside the box, second by the man outside the box. These two men will be observing the same thing but will, of course, give quite different descriptions. It is evident, too, that if we picture this falling box experiment performed on the sun, one observer being in the box and the other on the sun (naturally having the qualities of a salamander), they would give descriptions of things going on in the box which would be very different; but the differences between these would not agree with those noted in the first experiment performed near the earth. This is obvious, of course, because the gravitational field on the sun is so different, and the differences noted by the two men evidently depend upon this field. It is clear, then, that, if we knew exactly what these differences were, we would know all about the properties of the gravitational field. By means of mathematics we can discuss the forms which equations take in terms of one set of axes if they are given us in terms of another set, provided we know the relation between the two sets. In this case we do not know this relation; it is, in fact, what we wish to know in order to express our law of gravitation.

If I knew the exact law which described the manner in which the box fell at one point in a gravitational field, I would be able

to make this mathematical transformation; but, of course, since the field is not the same everywhere I would have to use different transformations. Einstein, who conceived the idea of the falling box, stated his postulate in a manner equivalent to saying that at any one point in a gravitational field the box when falling would annul all gravity effects for an observer inside. This he called his "Principle of Equivalence." The study of this isolated phenomenon would not, however, give a general law of gravitation. So the problem is to find the first special relation and then generalize it.

So Einstein, with his mind fixed on the thought that this law must have the properties we have discussed, investigated different possibilities. He finally obtained a relation which satisfied all of his conditions, and thus he was able to state his law of gravitation. I fear it would be too great a strain upon you for me to follow all his line of argument; but I can assure you it is most convincing.

Let us make a résumé of the matter. We are discussing laws of nature, that is, the statement in mathematical form of the generalizations from our experiments and experiences. We cannot formulate these laws until we have definite modes of measurement of different physical quantities. These measurements must all refer to a definite system of coördinate axes; but, according to the principle of relativity the form of the law must be independent of the particular system used. It is necessary, therefore, to study those mathematical expressions which satisfy this condition; and, in order to select the proper one for a law of gravitation, we use as limiting conditions a Galilean system and the case of slow velocities, because for these conditions we know the experimental facts. We thus finally obtain a law for gravitation in a most general form. When we apply it to special cases, it is found to describe the motion of bodies falling near the earth and the motions of the moon, of comets and of the planets, because Newton's law is a special case of Einstein's. The latter, though, describes in a most satisfactory manner the motion of the planet Mercury, which Newton's law did not; and it also allowed Einstein to make a prediction in regard to the deflection of rays of light in an intense gravitational field such as exists near the sun, a prediction which apparently has been verified. I shall return to this last subject in a few minutes.

You will thus have seen, I hope, that Einstein has not proposed any theory of gravitation unless you wish to call a theory of this universal force his exposition of how subjective the whole concept is. I mean by this that the fact of our being always thinking of gravitation, of our always feeling and perceiving its effect, is due to the fact that we are living on the earth; if we were in our falling box, we would be absolutely unconscious of the whole phenomenon. In an absolute sense, therefore, we cannot say that it is real. This is a most important fact to recognize. As a theory of gravitation, it states that there is no reality to it.

Now in regard to the deflection of rays of light in a gravitation field, there are several things to be said. The first one should be that the facts of the rays being bent is evidence that the velocity of light is changed by a gravitational field, and this is in conflict with one of Einstein's first postulates. This need not trouble us, however, for to make even the faintest appreciable change in this velocity a great change in the field of gravitation is required, and such changes we personally never experience here on the earth. But you can easily see why one who is on the earth would observe a ray of light to be deflected from a straight path on coming into the field of gravitational force near the sun. Let us again recall the idea of the falling box. As has been explained, to an observer inside the box a projectile fired from a gun would appear to move in a straight line, so would a ray of light. Therefore, if it were possible to fire a projectile with a velocity as great as that of light, the observer can be imagined as watching the projectile and the ray moving side by side, as it were. Now to an observer at rest on the earth, the paths of all projectiles appear curved, so would a projectile near the sun, like a comet; therefore the path of the ray of light must appear curved, since it is parallel to the projectile. The only points to note are that, of course, the curvature of the path is extremely small, and a very intense field of gravitation is required to produce a curvature which we can actually detect, and also that, in order to predict how much this curvature is, we must know what it would be for a projectile moving so rapidly. Newton's law obviously should not be applied, as it could be for ordinary projectiles from guns; but Einstein could use his law, and he did. The calculation seems to agree well with observation. You may ask how do we observe a minute deflection of the ray of light; but the answer is really

very simple. In fact, when I tell you, it will appear obvious. Consider the rays of light coming to us from a distant star; if we wish to receive them in a telescope, we point the instrument towards the star; if now the rays have been deflected, they will come to us from a different direction, and we shall have to turn the telescope until it points in the direction from which the rays are now coming. As you see, it is a very simple process.

I wish to call your attention to another very important deduction from Einstein's formulæ; but before doing this something should be said in regard to the general subject of theories, hypotheses and postulates. How are we to decide whether or not to accept them, to believe that we have attained truth? Any hypothesis, or postulate, is as we have seen, such a simple, such a general statement of belief that it is, as a rule, impossible to test it directly, we must resort to deductions. So what we do is to draw as many conclusions as possible from the postulates, conclusions which permit us to apply to their study our experimental methods. Then we test thoroughly these deductions. If they are verified, well and good; but if any one of them is not in agreement with experience we know that the postulates are not the proper ones. But, even if the deductions are apparently verified, we must always remember that our experimental methods and our best apparatus are very imperfect, and that our measurements are always accurate only to a certain percentage. Thus, if I am measuring the thickness of a wire or of a bolt, I am as a rule satisfied if I have an accuracy of one-thousandth of an inch; but, if I had two wires of the same thickness to within these limits, I would not say that they had identically the same thickness. A further consideration is the question as to whether or not there may not be other postulates which would lead to the same conclusions. Naturally this is an extremely difficult matter to settle, and we can never be convinced that we have disposed of all possibilities.

In the course of the development of a law for gravitation it is a consequence of Einstein's method that we learn something about the geometry of the space in which we live. As you know, Euclid and later mathematicians developed a science of geometry, in which, starting with certain axioms, postulates and definitions there is built up an elaborate structure of deductions. Thus circles, spheres, parallelograms, triangles, pyramids, cones, etc.,

are defined, and propositions are proved in regard to lengths, areas, angles and volumes. Every one knows certain of these theorems, *e.g.*, the one which states that the length of the circumference of a circle bears a constant ratio to its diameter designated by the symbol π and equal approximately to 3.14159, the one which states that the area of the surface of a sphere equals π times the square of the diameter, the one which states that the sum of the angles of a triangle equals two right angles, etc. I fear, though, that few people realize that all this work of Euclid and other geometers is a figment of the brain only. Its importance lies primarily in its being an illustration of a mathematical method. What I mean is this: suppose I were to do as most of you have done at school, take a string, fasten one end to a pencil or piece of crayon, then, holding the other end stationary on a blackboard or piece of paper, make the pencil or crayon describe a circle. Now, by my measuring rod let me *measure* the lengths of the diameter and of the circumference; will I find the ratio of these numbers to be π? Geometry has nothing to answer, it is not concerned with circles as we actually measure them. What geometry does is to say that if certain clearly stated axioms and postulates are granted certain conclusions are definite. Again suppose by mechanical means I make a sphere, and then measure the area of its surface, its volume and its diameter, will I find for my measurements the same ratios as those given by Euclid? I may or I may not. If I do not, then I know that the axioms or postulates made by Euclid are not a true picture of the space in which I am living and make my measurements. A kind of space in which Euclid's deductions would be verified by measurements would be called Euclidean space. Suppose we learn as a result of measurements of circles, spheres, etc., that our space is non-Euclidean, how can we discover what its properties are? This question may look at first rather hopeless, but I think I can explain by a few illustrations how we attempt to answer it.

We can picture a being who is himself flat like a visiting card, and who lives in a flat surface like the top of a large table, but unlimited, stretching out indefinitely. We can call this flat land; and the being living in and moving about in it is entirely unconscious of everything except what he meets in this surface, he cannot see anything above or below. He can build up a geometry of plane surfaces, circles, rectangles, triangles, etc. In particular,

he can start from any point and lay off lines of the same length in many directions from this point, like spokes of a wheel, and when he has drawn enough, he can make a circle by drawing a line connecting the ends of these lines, like the rim of the wheel. By laying off from the same centre longer radii he can describe other circles; he can measure the length of the circumferences and that of the diameters, and certainly if he is living in flat land, the greater he made his radius the longer would be the circumference of its circle. There is no apparent limit to the process. Further, if he wishes to describe the location of any point in his flat land, he has the means at hand; all he has to do is to number his concentric circles—in any way he wishes to, of course, having no duplicate numbers—and similarly number his radii. Then, if he says that a certain point is on circle twelve and radius fifteen, its location with reference to his starting point, the common centre, is definite. He simply has to give two numbers, so the flat-land is called a two-dimensional space. Again, suppose there is a sphere, a large one if you prefer, and suppose there is a being living in its surface, so he must have the shape of a little metal cap which fits the surface and slides over it. He can also start out from any point and lay off lines of equal length on the surface and can draw a circle connecting the ends of these lines. He can in this manner draw as many circles as he wishes; and again he can describe the location of any point on the surface with reference to his starting point by giving numbers in any arbitrary way to his lines drawn from this point and his circles. Again only two numbers are required, so he is living in a two-dimensional space. Bear in mind that this being knows only what his measurements tell him, he doesn't know anything about his land being a sphere, as we call it. Let us see what he could learn. First of all, his land has no limits; he never comes to an edge or a jumping-off point, any more than the man who lives in flat land. But he would observe that, after he had made his radius from his starting-point greater than a definite amount, the lengths of his circles became less and less; and, finally, if he made these radii all drawn from one point of just the right length, their ends would all coincide. In fact, he would have found the opposite pole. All this would be very interesting to him, but not surprising, because this is the only space he would know about.

The importance of these two pictures lies in the fact that it is

evident that all that any being knows of the space in which he lives comes from the measurements he can make of geometrical figures which he can describe in his space. Now, of course, we know that we are living in a space of three dimensions, because, as I have explained before, if I wish to describe the location of any point in this hall, all I need are three numbers. But what kind of a space of three dimensions is it? Remember that both of the beings whom I have pictured, the one in flat-land and the one in the surface of a sphere, knew they were living in two-dimensional space; but their experiences were quite different. If the latter were asked by another being in the same surface: "What kind of a space is this in which we are living?" what would be the answer? Simply that, "The space is evidently two-dimensional and my measurements tell me certain things." There could be no picture given or any other description. Now, so it is with us, if you ask me what kind of a space is this in which we are living, all I can answer is that it is three-dimensional and that our measurements tell us certain facts. I cannot give you any picture. But what are the facts in regard to our measurements? In the first place we have learned by careful work that, so far as our measuring instruments permit us to know, the deductions of Euclid are verified. That is, when we measure the angles of a triangle and add them we obtain the equivalent of two right angles; the ratio of the length of a circumference of a circle to that of its diameter is π, etc. But we acknowledge that our measurements are not very exact; the real facts might be that Euclid's geometry is not verified, there being discrepancies so slight that we cannot prove their existence. The best we can say is that our space is certainly very nearly a Euclidean three-dimensional one.

The bearing of all this discussion upon our present consideration of Einstein's theory of gravitation is that his method enables us to say something with more or less certainty about the measurements of geometrical figures—circles, triangles, etc.—here on the earth. That is, Einstein's theory predicts the metrical properties of figures in a gravitational field. When we apply this theory, for instance, to the measurements of a circle, it appears that, according to it, the ratio of the measured length of the circumference to that of the diameter is not exactly π; but the difference between the two quantities is so small that in actual

practice we could not detect it. Thus our statement made above in regard to the space in which we are living must, in accordance with Einstein's theory, be changed slightly, so as to be: we are living in a space of three dimensions, nearly but not perfectly Euclidean, and its properties can be expressed by certain mathematical relations. This is the best I can do, or the best any one can do, in giving a description of our space. The facts in regard to those relations are of interest to the mathematician or physicist only.

I must now conclude my lecture. My time and your patience are exhausted. I have indicated what I have thought were the essential features of Einstein's work, and have omitted many things which to a physicist are of supreme importance. I trust you have realized the novelty of Einstein's method, and I am sure you have been surprised to see what great consequences such apparently simple postulates have. To one who reads Einstein's papers with care, each page arouses admiration and inspires delight.

Hydrogen Ion Concentration of Pure Water.—H. T. BEANS and E. T. OAKES of Columbia University (*Journal of the American Chemical Society*, 1920, xlii, 2116–2131) have redetermined the hydrogen ion concentration of pure water and found it to be 1.23×10^{-8} or $10^{-7.91}$.

J. S. H.

Atomic Weight of Aluminum.—This constant of nature has been redetermined by THEODORE W. RICHARDS and HENRY KREPELKA of Harvard University (*Journal of the American Chemical Society*, 1920, xlii, 2221–2232). From the ratio of aluminum bromide to metallic silver, they find the atomic weight of aluminum to be 26.963.

J. S. H.

Mechanism of Bating.—JOHN H. WILSON (*Journal of Industrial and Engineering Chemistry*, 1920, xii, 1087–1090) has made a study of bating, which is one stage in the manufacture of leather. For a bate, use was made of pancreatin or trypsin. Two distinct phenomena were produced by bating. The swollen limed skins were reduced to a condition of minimum swelling; and the elastin fibers, which are present in the outer layers of the skin, were digested.

J. S. H.

Long Electrical Transmission Lines in Europe. (*La Nature*, October 25, 1920.)—In this matter the United States of America holds the record with three lines almost 400 km. (250 miles) in length, that of the Au Sable Electric Co. in Michigan, 394 km. long, operating at 140,000 volts, the slightly shorter line of the Pacific Light and Power Co., Los Angeles, Calif., at 150,000 volts, and finally the system of the Southern Sierra Power Co., also in California, at 140,000 volts. In Europe Spain has the longest line. It runs from Molinar to Madrid and is 255 km. long. Next comes the Italian line from San Dalmazzo to Novi, 250 km. A second Spanish line from Molinar to Carthagena has a length of 213 km. All three of these are employing 70,000 volts, but two important lines supplying Barcelona, Spain, are at 88,000 and 110,000 volts, respectively.

In France there are two lines about 180 km. in length, one connecting Lyons to Bozel in the mountains of Savoy and employing the Thury direct-current system of transmission, while the other leads from Ventavon to Arles on the lower Rhone, the voltage being 50,000. There are several other French lines ranging from 150 to 160 km. in length. One of these from Ugine to Lyons transmits at 77,000 volts, thus far the highest A.C. voltage used in France for transmission purposes. There is, however, a line under construction from Olton Goesgen in Switzerland to Pouxeux, 156 km., which will operate at 100,000 volts, while the railroad company of the Midi is building a line to Bordeaux at 120,000 volts. G. F. S.

Addition Compounds of Sulphuric Acid and the Neutral Sulphates of the Alkali Metals.—By means of the freezing point method, JAMES KENDALL and MARY L. LANDON of Columbia University (*Journal of the American Chemical Society*, 1920, xlii, 2131–2142), have studied the formation of compounds in systems containing 100 per cent. sulphuric acid and an anhydrous neutral sulphate of an alkali metal: potassium, ammonium, sodium, and lithium. Each system was studied from its eutectic point up to a temperature of approximately 300° C. Both potassium sulphate and ammonium sulphate united with sulphuric acid to form two addition products; one molecule of sulphate was combined with one molecule of acid in one of these addition products, and with three molecules of acid in the other addition product. Sodium sulphate yielded three addition compounds, in which one molecule of that salt was combined with one, two, and nine molecules, respectively, of sulphuric acid. Lithium sulphate also yielded three addition products; however, one molecule of the salt was combined with one, two, and seven molecules, respectively, of sulphuric acid. Several of these addition compounds existed in two or more modifications.

J. S. H.

RECENT ATTAINMENTS IN WIRED RADIO.* [1]

BY

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I. INTRODUCTION.

PUBLICATIONS in the recent technical press have revealed that the problem of "Wired Radio" has aroused widespread interest within the last few years, both in this country and abroad, and in its many practical and theoretical phases has been the subject of extensive study and investigation.

The publication in 1911 by Major (now Major-General) George O. Squier, of a paper on "Multiplex Telephony and Telegraphy," [2] marked an epoch in the scientific progress of the telephone and telegraph art. Though the problem of multiplex telephony and telegraphy had engaged the attention of a number of inventors prior to General Squier's work, the ideas underlying the schemes which hitherto had been proposed were based, almost without exception, upon the principles and practices of the old telephone art. The great advance made by General Squier, and that which made the use of high (radio) frequency carrier currents successful, was the combined utilization of tuned circuits, and a detector or rectifier at the receiving terminus. By this achievement the principles common to the radio art were made available to the older wire systems and a new method of transmission of intelligence was created.

The utility of wired radio was early realized in this country by certain of the telephone and telegraph companies, and its practical development in the realm of low frequency carrier currents has been carried to an advanced and commercial stage; long-distance telephone and telegraph lines employing the system are in daily operation between a number of large cities. That this country is not alone in the development of this new art is evi-

* Communicated by Major-General George Owen Squier, Chief Signal Officer, U. S. A., Member of the Institute, Associate Editor.

[1] "Wired Radio" refers to the use of high frequency currents guided by wires.

[2] *Trans. A. I. E. E.*, Vol. 30, 2nd Part, p. 1617, 1911.

denced by the publications of Wagner,[3] Faszbender and Habann,[4] in Germany, and by Marumbo [5] in Japan. The two former papers contain interesting accounts of the development of wired radio apparatus in Germany during the war; the latter paper, though quite brief, is also of great interest, as it contains descriptions of methods and apparatus used where a high voltage power line was employed for transmission means.

II. PRELIMINARY EXPERIMENTS.

The practical development of wired radio was not pushed vigorously during the war by the Signal Corps, U. S. Army, as military requirements demanded apparatus of a more purely radio nature. A limited number of field telegraph sets operating on low frequencies were developed and manufactured. In an attempt to find use for a large quantity of radio telephone apparatus which had been purchased during the war, and which was then idle, experiments were commenced in the early part of 1919 with a view to investigating the possibilities of adapting this apparatus to wired radio communication. A brief account of these preliminary experiments has already been published in the JOURNAL of this Institute.[6]

In the experiments described, it was originally intended to employ frequencies of the order of 75,000 cycles per second for carrier purposes since in common with the prevailing opinion it was believed that the excessive values of attenuation which, it was thought, must occur at the high frequencies, would be prohibitive of transmission over anything but the very shortest distances. Accordingly, efforts were first directed towards altering the constants of the transmitting apparatus so that frequencies of this order of magnitude could be obtained. However, the radio telephone apparatus which was to be employed was originally designed for operation at high frequencies (short wavelengths) and on relatively small antenna capacities; from experiment it was found that if the original intention of using low frequencies was adhered to, a considerable change in the construction of the apparatus would be required. As a consequence

[3] Wagner: *Telegraphen u-Fernsprech Technik*, No. 3, p. 29, June, 1919.

[4] Faszbender and Habann: *Jarhb. d. draht. Tel. und Tel.*, Band 14, p. 451, October, 1919.

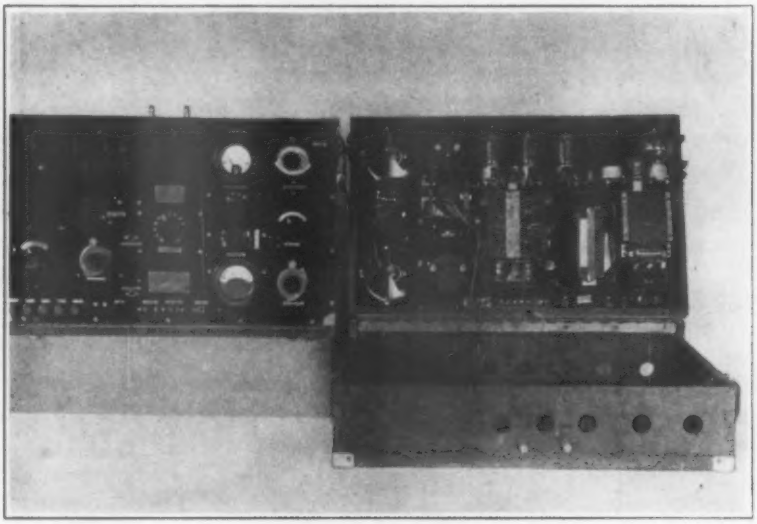
[5] Marumbo: *Proc. I. R. E.*, Vol. 8, No. 3, p. 199, June, 1920.

[6] J. O Mauborgne, Vol. 188, p. 91, 1919.

it was decided to abandon the low frequency idea and to utilize, what had heretofore been considered the extremely high frequencies, as a carrier means.

One distinct advantage which was anticipated and which does result from the use of very high carrier frequencies is the elimination of speech distortion so common to long-distance wire telephony. In accordance with radio telephonic theory, three frequencies will be present in load circuit (antenna or telephone

FIG. 1.



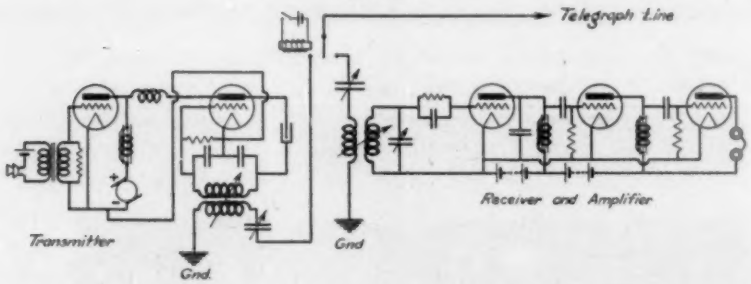
lines), if the modulating frequency contains no harmonics, *viz.*, respectively, the main carrier frequency, and the sum and the difference of the main carrier frequency and the modulating frequency. If harmonics are present in the modulating frequency, then in addition to the three main frequencies, there will be additional frequencies corresponding to the sums and differences of the main frequency and each of the harmonics. Assuming that the fundamental frequency of the voice is 800 cycles per second, and the limiting harmonic to be 3000 cycles per second, it is readily seen that at high carrier frequencies, the band of frequencies which is transmitted, is so small a percentage of the main carrier frequency that the attenuation to all frequencies

within the band is practically the same. The elimination of speech distortion due to this cause thus automatically results.

Views of the transmitting and receiving apparatus employed in the experiments are shown in Fig. 1; a diagram of connections of the circuit is given in Fig. 2.

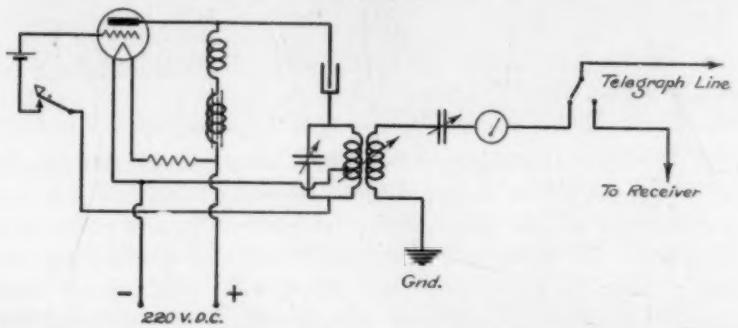
The experiments were conducted on a wire belonging to the

FIG. 2.



Postal Telegraph Company, running between Washington and Baltimore, a distance of 66 wire miles (106 Km.). The line was aerial throughout the distance of transmission and contained a single section of overhead cable approximately one-quarter of a mile in length; in the experiments the transmitting and receiving

FIG. 3.



apparatus was connected between the line and the ground. The carrier frequency employed was 600,000 cycles per second. Throughout the entire tests the line was in ordinary duplex telegraph operation; no interference, either from the ordinary telegraph operation, or from the wired radio, was experienced by either system. The normal range of the apparatus when

functioning as a " radio " telephone in communication with similar apparatus, is ten miles; it is seen that by the methods of wired radio, the range of communication was increased many times.

For telegraph purposes, only a vacuum tube transmitter was developed for operating directly from a 220-volt direct current lighting source. A diagram of connections of the apparatus is shown in Fig. 3.

The results of these preliminary experiments were of unusual interest, as it was demonstrated that transmission over relatively great distances could be accomplished with frequencies greatly in excess of the value which had heretofore been taken as the upper limit. From a practical standpoint, a number of questions arose requiring further investigation, of which probably the most important was that of determining the relative parts played by radiation and by true conduction, and of the distances over which satisfactory transmission might be expected to be obtained.

III. LATER EXPERIMENTS.

Further investigation along this line was made possible through the courtesy and coöperation of the New York Central Railroad Company. Through their General Superintendent of Telegraphs, Mr. E. C. Keenan, a pair of telephone lines running between New York City and Albany, N. Y., and two railway passenger coaches, stripped of their seats, were placed at the disposal of the Signal Corps. These cars were equipped with special and standard types of radio telephone and telegraph apparatus and auxiliary equipment, storage batteries, receiving and amplifying equipment, etc. An oscillograph was included as a part of the equipment of one car, for the purpose of studying telephonic modulation. During the actual field tests the two cars served as the abode of the members of the party. Views of the two coaches and an interior view of the coach which served as the fixed station, are shown in Figs. 4 and 5.

Experiments on the New York Central telephone lines were commenced the latter part of 1919. They had as their primary object a study of the possibilities of establishing communication over fairly long sections of line when employing extremely high carrier frequencies. The electrical properties of the telephone lines when serving as the load on the radio frequency generator were also to be studied, since from the information thus obtained,

FIG. 4.

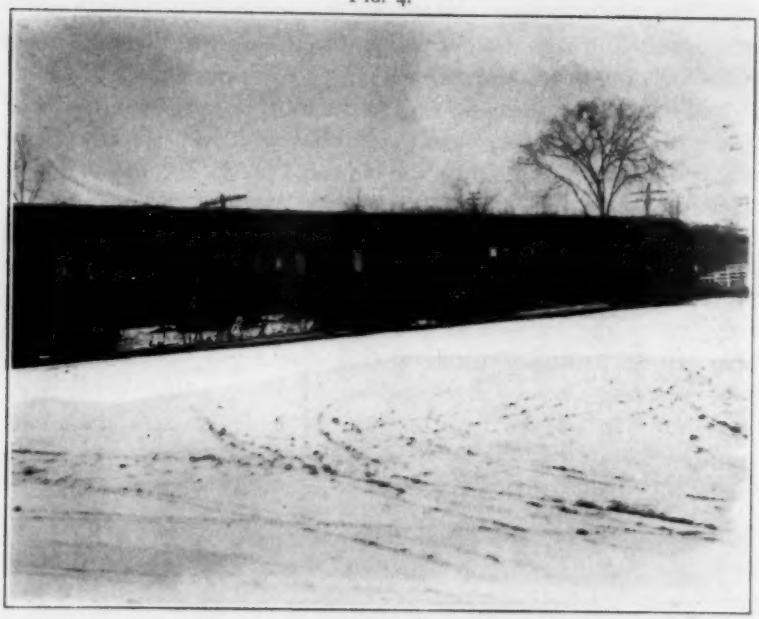


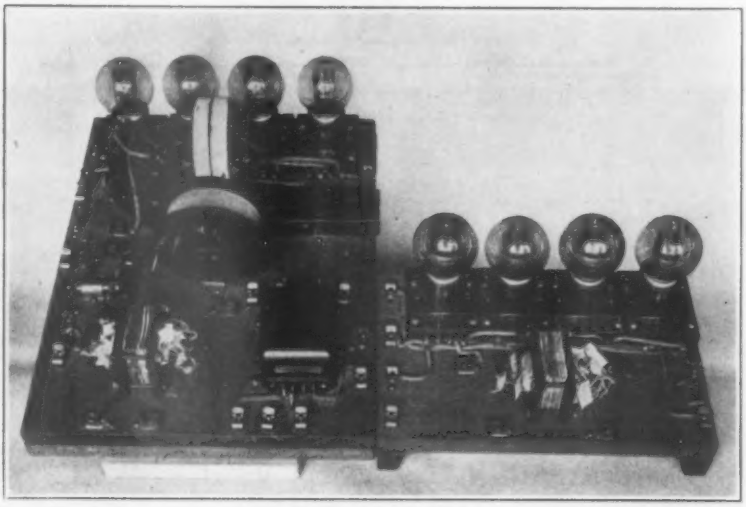
FIG. 5.

artificial line loads might be constructed upon which the development of future transmitting and receiving equipment could be based.

The transmitting apparatus employed throughout these tests consisted of that shown in Fig. 1, and in addition, special experimental apparatus shown in Fig. 6.

Two arrangements of the special transmitting apparatus were employed, one in which the telephone lines were coupled directly to the radio frequency generator circuit and one in which the

FIG. 6.



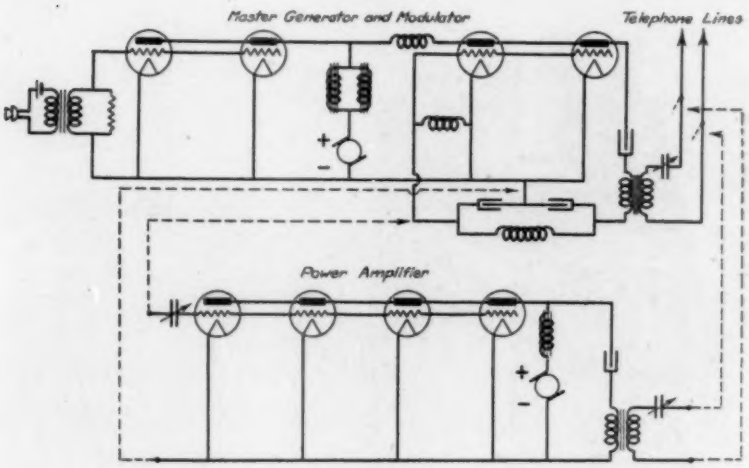
modulated radio frequency power was first amplified by a bank of vacuum tubes connected in parallel, and then fed into the line load. The generating and modulating apparatus is shown at the left in Fig. 6; the power amplifier is shown to the right. The diagram of connections of the generating and modulating circuit when operating alone into the lines and when serving as the "master" set of the amplifier equipment, is shown in Fig. 7.

The type VT-2 vacuum tube was used for transmitting, two operating in parallel, in both the generating and modulating systems, and from one to four tubes in parallel in the power amplifier. Plate voltage for operating the transmitting tubes of the fixed station was furnished by a motor generator set driven from

110-volt power lines; in the mobile car plate voltage was furnished by dynamotors operating from storage batteries. The transmitting apparatus was arranged for telephonic operation and for both continuous wave and buzzer modulated telegraph operation.

The method of connecting or coupling the line load to the radio frequency generating unit is novel and possesses several features which are of value in duplex telephony. One annoying source of trouble in duplex telephony is interference produced on the receiving apparatus by the stray electric and magnetic fields

FIG. 7.

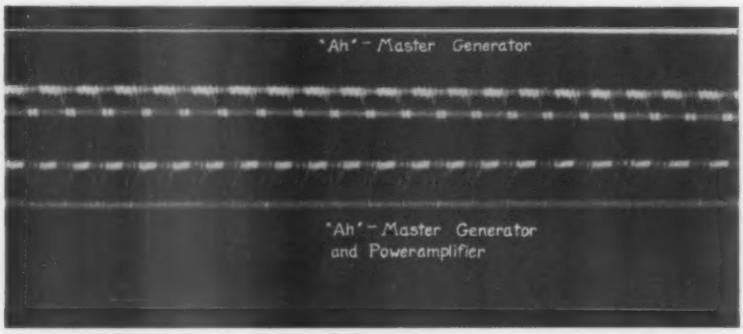


of the transmitter. Experiment [1] has shown that the electric field may be confined to within known limits by shielding of the apparatus; the magnetic field, however, cannot be restricted by shielding and its disturbing effects may be minimized only by the proper design and displacement of the inductive and current carrying elements of the circuit. In the present circuit, energy is furnished to the lines through the means of a closed iron core transformer whose primary is connected, in series in the plate circuit, with the "frequency determining circuit." This transformer is designed to have a minimum magnetic leakage and its use permits of the further use of an inductance, in the frequency

[1] Weinberger and Dreher: *Proc. I. R. E.*, Vol. 7, No. 6, p. 584, 1919.

determining circuit, of a compact form which has a very restricted magnetic coupling field. The requirement for a minimum magnetic disturbance due to this inductance is equivalent to the requirement of a minimum value of ampere turns of the inductance effective at a given point in space. A theoretical consideration of this type of circuit shows that for a given frequency, and constant number of turns in the frequency determining inductance, the disturbing magnetic field will be a minimum and the line current a maximum when the ratio of the capacities interposed respectively between plate, grid and common negative point of filament is made large. When this is the case, the effective

FIG. 8.



resistance of the frequency determining circuit is small and the major portion of the high frequency power converted by the vacuum tube is expended in the load. Experiment confirms these conclusions. The increase in ratio of the plate to grid capacities in this type of circuit, aside from other practical considerations, is limited by its effect upon the telephonic modulation; it was found that if carried to an extreme, the degree of modulation was considerably reduced. The iron core transformer is seen in the foreground of the photograph of the generator unit in Fig. 6.

Oscillograms of the modulated high frequency output obtained from the generator, acting singly, and in conjunction with the power amplifier, are shown in Fig. 8.

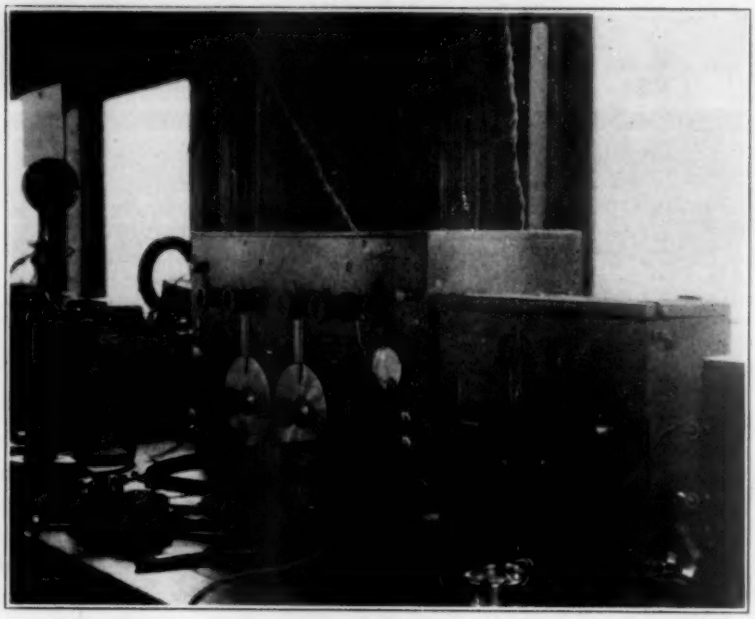
The curves therein represent the envelop of the maximum amplitudes of the radio frequency currents, which are varied in accordance with speech frequencies; the straight lines about

which the curves vary represent the unmodulated current. The oscillograms were obtained when the word " ah " was spoken into the microphone in a somewhat loud tone of voice.

The receiving equipment, shown in Fig. 9, consisted of a standard radio receiver and a two-stage audio frequency amplifier. The type *VT*-1 vacuum tube was used throughout.

One method of connecting the receiving apparatus to the telephone line is shown in Fig. 10. The apparatus employed in the

FIG. 9.

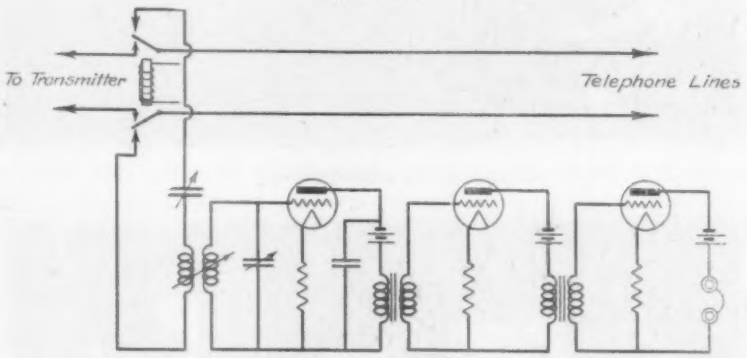


later experiments was entirely experimental in character and was designed to obtain a high degree of flexibility in power output and in frequency. It was intended in no manner to represent a finished product.

In adjustment of both the transmitting and receiving apparatus, the lines were tuned to resonance by means of the series line capacity. The tuning, while not as sharp as obtained with a radio antenna, revealed a distinct resonant point, and could be made broad or sharp at will, by altering the magnitude of the loading or coupling inductance.

The transmission and reception tests were conducted on a pair of telephone lines running from New York City to Albany, N. Y., paralleling the Putnam and Harlem Divisions of the New York Central Lines, and the Boston and Albany Railroad for a short distance into Albany. The lines were aerial throughout the length effective in these tests and were transposed approximately every half mile; short lengths of overhead cable were included at three intermediate points where the lines entered stations for test purposes. These lines constituted the main telephonic channel of the New York Central Railroad between New York City and Albany and were in almost continuous operation. Besides being in ordinary telephonic operation the lines

FIG. 10.

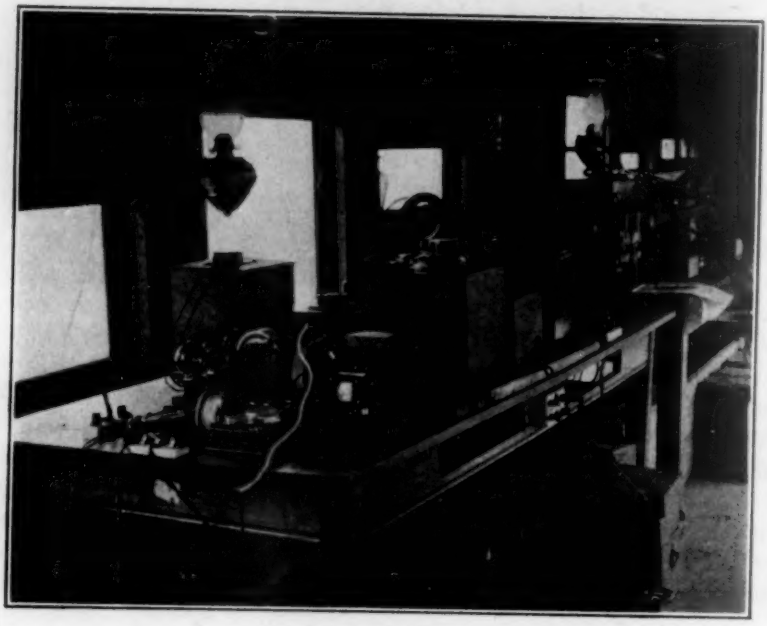


were composited, each wire of the pair operating a Morse circuit to ground. Throughout the experiments no interference with either system was produced by the other.

Telephonic and telegraphic communication was established over the pair of wires from Elmsford, N. Y., just north of New York City, and Albany, N. Y., a distance of 130 miles (209 Km.). A good quality of speech and both continuous wave and buzzer modulated telegraph signals were obtained. The carrier frequency varied from 600,000 to 300,000 cycles per second (wavelength 500 to 1000 metres). The high frequency power output varied from less than one-half watt to twenty watts, depending upon the apparatus used. It is to be observed in this respect that when dealing with radio telephone circuits, in contradistinction to those used in radio telegraphy, an indication of maximum cur-

FIG. 11.

Apparatus in fixed station.



Apparatus in mobile station.

rent into the load is not always indicative of most efficient operation. What is sought and that which is the most effective at the distant receiving station, is the faithful and complete telephonic modulation of the power output that is available. A large power output incompletely or imperfectly modulated is no more effective than a smaller one more completely modulated and modulated with less distortion. Views of the transmitting and receiving equipment of both the fixed and mobile stations are shown in Fig. 11.

In the Washington-Baltimore experiments, as well as in the later ones, evidence of the existence of standing waves on the telephone lines was obtained; this was quite noticeable in the moving-train experiments later described. Standing waves, as is well-known, are produced by reflection from either the far end of the line, or from intermediate points where there occurs a sudden transition in the electrical properties such as produced by the introduction of short lengths of cable, changes in the size of conductor. In the latter case, unless the reflection is complete, a portion only of the outgoing wave will be turned back, and a portion will proceed unreflected; as a result there will occur partial reflection and partial unreflected transmission. The location of the receiving apparatus with respect to the transmitter, when standing waves are present, is therefore determined largely by the wave-length or frequency of the carrier wave, since for efficient reception, it is necessary to avoid both voltage and current nodes. It was found that altering the frequency at the generator terminal apparently did not produce an effect in moving the nodes and loops along the lines equivalent to that of moving the receiving apparatus.

In the preliminary experiments previously mentioned, and in the earlier New York Central tests, one overhead wire with a ground return was employed. A comparison of this arrangement with an all-metallic circuit showed that the latter was the much superior of the two, both as to volume of speech received and elimination of external disturbances such as that produced by nearby radio stations. The reason for this improvement is attributable to the pronounced energy radiating and absorbing properties possessed by the one elevated wire with ground return. Both the direct current and " skin effect " resistance of a metallic

loop should be equal to approximately twice that of one wire and ground. Theory indicates, however, that the resistance introduced by radiation is proportional, among other factors, directly to the effective distance between the conductors, which in the case of an elevated wire with ground return, is equivalent to the distance between the wire and its "electrical image" in the earth or to twice its physical height above the ground. This distance, of course, is many times greater than the spacing between the two metallic conductors and hence at frequencies where the radiation plays an important part, the effective resistance of the latter system should be much less than the wire-ground system. This conclusion is borne out by the tests referred to and by actual resistance measurements which are described later on.

The question of cross-talk was investigated, as it was believed that at the high frequencies inter-line disturbances might be prohibitive of utilizing the same carrier frequency on more than one line or pair of lines on the same pole. In the Washington-Baltimore tests, it was found that it was possible to receive signals with the lead wire to the receiving apparatus disconnected from the line in question and held in the immediate vicinity of the same or wrapped around, but not making contact therewith. Signals of reduced intensity were also received with the lead wire connected to other immediately adjacent lines on the same cross-arm. Complete elimination of cross-talk to other lines, however, was obtained when the first few miles of conductor adjacent to the transmitting apparatus ran through underground cable. When employing an entire metallic circuit, at the very highest frequencies used, there was a complete absence of cross-talk to the other conductor pairs on the same pole line.

The problem of multiplexing the lines was also studied, but limitations in time available for field operations did not permit of utilizing the degree of thoroughness which the successful solution of this intricate problem demands.

As a prerequisite to the solution of the main problem of multiplex there are the solutions of the two allied problems of multiple transmitter operation and duplex telephony; by duplex telephony is meant simultaneous transmission and reception, accomplished without the assistance of switching devices. The difficulties encountered in multiple transmitter operation are not serious and

are easily overcome; those encountered in duplex telephony, however, if the band of frequencies available is restricted, are most serious, and become almost insurmountable if the receiver sensitivity is at all heightened by the use of radio frequency amplifiers. Utilizing artificial line loads, the design and construction of which is described elsewhere in this paper, the problem has been successfully solved in the laboratory at the present writing, only by paying the most strict attention to details of apparatus, design, construction, and shielding.

IV. MOVING-TRAIN EXPERIMENTS.

A very interesting application of wired radio is that of establishing communication with a train while in motion. The utility of such a system for insuring greater safety of travel, transmission of business information, etc., is readily realized. In 1914 and 1915 experiments were carried on by the Delaware, Lackawanna and Western Railroad with a view to establishing such a service by *radio*. According to reports published at that time [8] transmitting powers in the neighborhood of one kilowatt and antennas averaging 150 feet in height and between 200 and 400 feet in length were employed. With this arrangement, telephonic communication of a more or less satisfactory character was obtained between a moving train and a fixed station for distances up to 50 miles (80 Km.). It was reported that the directional properties of the transmitting and receiving antennas utilized in these tests were very noticeable; when the train was in movement over a section of track containing numerous curves, the received signals fluctuated greatly in intensity. As adapted for this purpose, wired radio utilizes the telephone conductors paralleling the railroad tracks as a properly directed path for the carrier frequency currents. The transmitting and receiving apparatus at the fixed station was connected between one aerial wire and the ground; in the moving station the apparatus was connected to a closed loop, properly orientated with respect to the telegraph wires. The transfer of energy from the elevated wires to the loop, or *vice versa*, is effected by the inductive action of the electromagnetic field. Employing this system and with high frequency powers in the neighborhood of 2 watts, an excellent quality and quantity of telephonic speech and telegraphic signals were ob-

[8] *Electrical World*, Vol. 66, p. 570, 1915.

tained between the two railroad coaches, one stationary and the other in motion, over distances ranging up to ninety miles (145 Km.). During the moving-train tests, the presence of standing waves on the lines was again verified; the signals received in the moving station varied periodically in intensity as the car was under way.

V. HIGH FREQUENCY LOAD CHARACTERISTICS OF THE TELEPHONE LINES.

Theory.

A knowledge of the electrical load properties of the two elevated conductors or of one conductor with ground return, when operating at the extremely high frequencies, is prerequisite to the design and development of both transmitting and receiving apparatus. *A priori,* it is known that the load resistance and reactance of the lines, *i.e.,* the effective resistance and reactance into which the radio frequency generator works, and in effect the internal resistance and reactance of the generator which furnishes power for operation of the receiving apparatus, are functions of the frequency. The exact manner of their variation with the frequency is implicitly contained in the standard theory of transmission lines, but so far as the author is aware, only a limited amount of experimental data has been published which tends to confirm the theory at the low range of high frequencies and practically none when extremely high frequencies are dealt with. With such information artificial lines may be constructed which will permit of a thorough study of the high frequency properties of transmission lines and upon which the development of transmitting and receiving apparatus may be based. The problem corresponds to the problem of measurement of the constants of a radio antenna.

In accordance with the theory,[9] a distributed circuit such as an elevated telephone line, of length l, possesses per unit length, inductance L, capacity C, resistance R and leakage conductance G. The ratio of the voltage applied at the generator terminals of the distributed circuit, to the current input at the same point, for the conditions of the far or receiving end open, and short-circuited upon itself, after the transient state has disappeared, is given by

[9] Malcolm: "Theory of Submarine Telegraph and Telephone Cable," Chap. vi. Pernot: "Electrical Phenomena in Parallel Conductors," Vol. I, Chap. viii.

the two following vector expressions. For the receiving end open-circuited:

$$(1)\ldots\ldots Z' = \sqrt{\frac{R+j\omega L}{G+j\omega C}} \cdot \coth \cdot l \cdot \sqrt{(R+j\omega L)(G+j\omega C)}$$

For the receiving end short-circuited upon itself:

$$(2)\ldots\ldots Z'' = \sqrt{\frac{R+j\omega L}{G+j\omega C}} \cdot \tanh \cdot l \cdot \sqrt{(R+j\omega L)(G+j\omega C)}$$

wherein $j = \sqrt{-1}$, and coth and tanh represent the hyperbolic cotangent and tangent of the complex angle indicated.

In transmission engineering Z is generally termed the "final sending-end impedance." If the line losses are assumed to be zero, *i.e.*, $R = G = o$, the impedance Z, for the two conditions degenerates into a reactance, and equations (1) and (2) may be written in the form:

$$(3)\ldots\ldots X' = -\sqrt{\frac{L}{C}} \cdot \cot \cdot l \cdot \sqrt{\omega^2 L C}$$

$$(4)\ldots\ldots X'' = \sqrt{\frac{L}{C}} \cdot \tan \cdot l \cdot \sqrt{\omega^2 L C}$$

Under the conditions of zero line losses, the line reactance effective at the transmitting apparatus is seen to be a function of the frequency ($\omega = 2.\pi.f$) since both the circular cotangent and tangent are periodic functions of ω, varying from minus infinity to plus infinity. From equations (3) and (4), it is evident that the reactance becomes zero respectively when:

$$(5)\ldots\ldots \omega = \frac{1}{\frac{2}{\pi N}\sqrt{l L \cdot l C}}$$

and

$$(6)\ldots\ldots \omega = \frac{1}{\frac{2}{\pi N'}\sqrt{l L \cdot l C}}$$

where $N = 1, 3, 5, 7 \ldots$; and $N' = 0, 2, 4, 6 \ldots$ The fundamental frequency is obtained when $N = N' = 1$, and is given by

$$(7)\ldots\ldots f = \frac{1}{4\sqrt{l L \cdot l C}}$$

The case of *quarter-wave* oscillating systems of relatively low losses, such as is closely approximated by the radio antenna, falls

under equation (3) and has been discussed extensively by Stone,[10] Cohen,[11] Miller,[12] and Wagner.[13]

It is, of course, impossible to obtain a line entirely free from losses, particularly at high frequencies, where, as theory indicates, the effective resistance due to skin effect and radiation may become very large. In this case equations (3) and (4) no longer represent the true condition of affairs and equations (1) and (2) apply. Both of these latter are vector equations involving the hyperbolic tangent and cotangent of the complex angle

$$[l \cdot \sqrt{(R + j\omega L)(G + j\omega C)}]$$

These two functions go from maximum to minimum values as the complex angle passes through multiplex of $\frac{\pi}{2}$, and hence it is to be expected that the impedance will vary in a somewhat corresponding manner. Equations (1) and (2) may be split up into real and complex components, respectively, representing the effective load resistance and reactance of the lines; the resulting expressions are, however, rather complicated and without substitution of actual numerical values are difficult of interpretation.

Measurements of the sending end reactance and resistance as functions of the frequency, of an artificial transmission line, whose receiving terminals were open, over a range of frequencies up to and including the third harmonic, have been described by Morecroft,[14] who obtained a periodic variation in the reactance from positive to negative values, and a pulsation in the resistance values.

In a distributed system in which the physical length is very great in comparison to a wave-length, it is evident that the line load may function either as a positive or negative reactance depending upon the frequency of the impressed emf; the resistance will also vary between certain maximum and minimum values.

The theory as is generally given requires modification when considered from the standpoint of high frequencies, since it is implicitly assumed that the four line *constants, viz.*, the unit resistance, inductance, capacity and leakage conductance are con-

[10] Stone: *Trans. Int. Elec. Congress, St. Louis,* Vol. 3, p. 555, 1904.
[11] Cohen: *Bul. Bu. of Standards,* Vol. 6, No. 2, p. 247, 1909.
[12] Miller: *Proc. I. R. E.,* Vol. 7, No. 3, p. 300, 1919.
[13] Wagner: *Arch. d. Elektrotecnik.*
[14] Morecroft: *Proc. I. R. E.,* Vol. 5, No. 6, p. 389, 1917.

stant and independent of the frequency. This assumption is quite valid when dealing with very low frequencies, but does not hold for all of the four line constants at high frequencies. Two agencies act to make this the case, *viz.*, the progressive unequal distribution of current over the conductor cross-section as the frequency is increased, and the finite velocity of propagation of the electromagnetic field; this velocity is taken to be equal to that of light, *viz.*, 3×10^{10} centimetres per second. The first of these phenomena, commonly known as "skin effect," causes the inductance of the conductor to decrease and its resistance to increase, as the frequency increases. Expressions have been derived and tables prepared [15] which permit the rapid calculation of the change in the inductance and resistance of straight wires for frequencies from 100 to 3,000,000 cycles per second. It is of interest to note in this respect that the inductance change with the frequency is relatively slight while the change in resistance may be very great. As an illustration of the changes predicted by the theory for the case of a pair of No. 9, A. W. G. hard-drawn copper wires, spaced fifteen inches, the inductance computed for the frequencies of 25,000 and 1,000,000 cycles per second were respectively 3.68×10^{-3} and 3.60×10^{-3} henrys per loop mile, or a change of 2 per cent.; the corresponding values of resistance at the two frequencies are very closely equal to 16 ohms and 90 ohms or a change of over 500 per cent. So far as is known the unit capacity of the lines is not affected by the change in current distribution with the frequency.

In low frequency alternating current work where the physical dimensions of the apparatus or circuits are small, in comparison to a quarter wave-length of the fields, the velocity of propagation is properly assumed to be infinitely great. At radio frequencies this assumption may or may not be permissible, depending upon the frequency and physical displacement of circuits. The effect which a finite velocity of propagation has upon the circuit constants has been investigated mathematically by Steinmetz,[16] who has arrived at the conclusion that of the four line constants the resistance was the only one to be affected when the frequency

[15] *Scientific Paper No. 169, Bureau of Standards*, pp. 172 and 226, 1916.

[16] Steinmetz: *Proc. A. I. E. E.*, March, 1919.

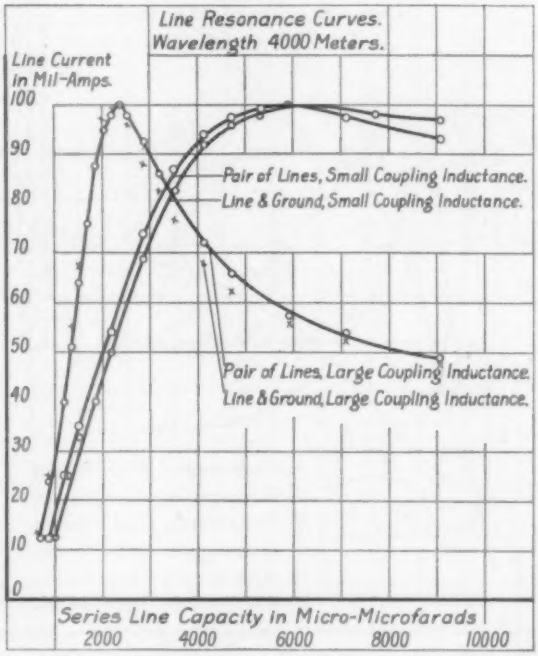
was increased to large values but did not exceed approximately 1,000,000 cycles per second. Expressions for inductance and capacity of a pair of lines or of one elevated line with ground return, as computed on a basis of finite propagation (when corrected for skin effect if necessary), were proven to be valid at high frequencies as long as the limit specified was not exceeded. The resistance was found to increase at a rate proportional, among other factors, directly to the spacing between the conductors and to the square of the frequency. For the case of a pair of wires, *i.e.*, a complete metallic circuit, this distance is comparatively small; however, in the case of a single conductor with ground return, the effective spacing between conductors is equal to the distance between the elevated conductor and its electrical image in the earth, or to twice its physical height above the ground. The radiating properties of both distributed systems should become the more pronounced as the frequency becomes higher; those of the wire-ground system should, for a given frequency, be more pronounced than of the two-wire system.

In the derivation of equations (1) to (4), the additional assumption was made that the lines in question were free in space and consequently were unaffected by the presence of other lines or by the nearby presence of the ground. These conditions, however, are rarely ever completely realized in practice; the lines used in these particular tests, throughout their entire length, were included on the same pole with other lines, the total number of lines in some locations running as high as one hundred or more.

As a brief summary it may be stated that, if the accepted transmission line theory does not suffer serious modification when corrected for change of inductance and resistance, and for a possible change of the leakage conductance, about which little is known, for increasing frequency, it is to be expected that the impedance of the lines, measured as functions of the frequency from the generator end, will undergo a periodic variation between maximum and minimum values, as the frequency increases. From a consideration of the theory as outlined briefly above, and from reported observations of measurements on artificial lines, it is to be further expected that the two components of the impedance, *viz.*, the effective reactance and resistance of the load, will undergo

a similar periodic variation, the former passing from positive to negative values, the latter pulsating between the maximum and minimum positive values. In the line measurements described in the following, because of the extremely low potentials applied to the line, it was impossible to accurately measure the impedance as would be computed from observed ratios of the applied voltage to the current input. Instead, the reactance and resistance of the line load were measured separately by the methods later described.

FIG. 12.

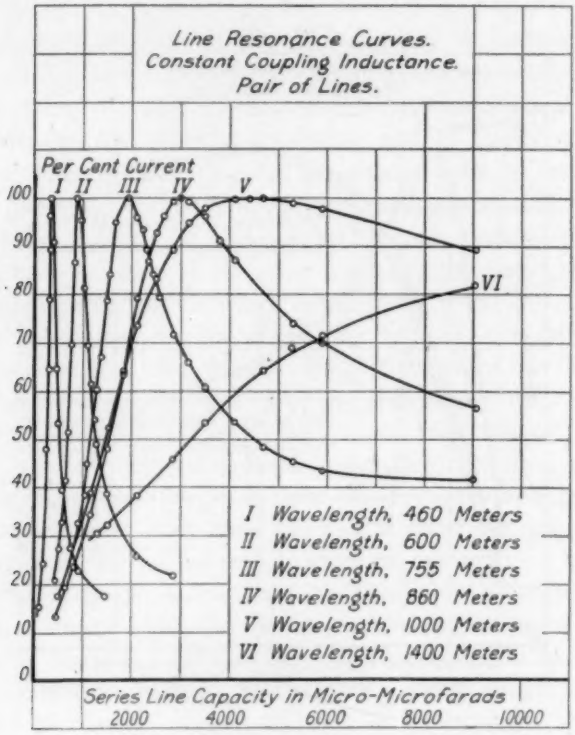


VI. LINE LOAD MEASUREMENTS.

In the adjustment of the transmitting apparatus during the transmission and reception tests, maximum line current was obtained by resonating the load, including both the lines and local circuit, to the operating frequency; this was accomplished by variation of the series line capacity. The tuning features of the line load are illustrated by the curves shown in Figs. 12 and 13. In the former are plotted values of line current against values of

series line capacity for both a pair of lines and for one line and ground, for two different values of coupling inductance, but with a constant frequency of 75,000 cycles per second (wave-length 4000 metres); as is to be expected, the tuning is sharper with a large coupling inductance than with a smaller one, the effect in the former case being to "stiffen" the line. The same

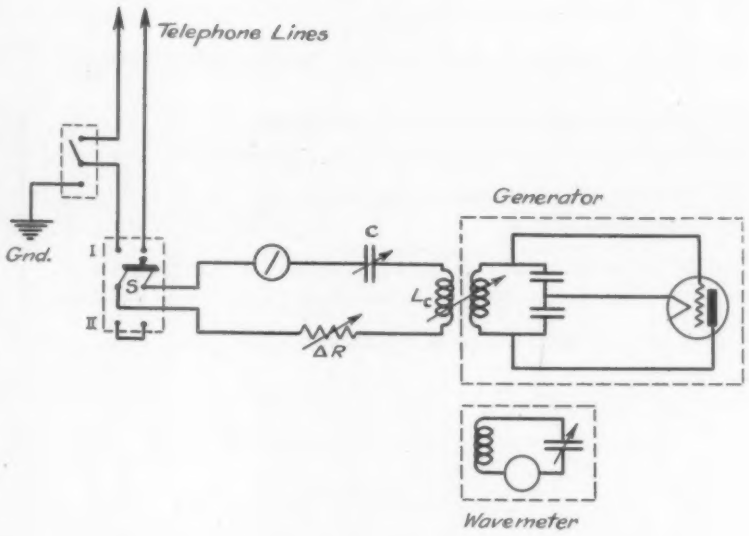
FIG. 13.



result is to be accomplished by maintaining a constant value of coupling inductance and varying the wave-length; as shown in Fig. 12, the line tuning becomes broader as the wave-length is increased. The ordinates of the curves in this figure were computed on the basis of assigning a value of 100 per cent. to the observed maximum values of each curve and computing the values of the other points as a percentage of 100.

The reactance and resistance measurements were also based upon the phenomena of resonance. Referring to Fig. 14, wherein is shown the circuit employed during measurement, it will be observed that the line load in series with a variable capacity C and resistance ΔR, is inductively coupled to the radio frequency current generator through the inductance L_c. With the resistance ΔR, set to zero, and with the switch S in position I, the capacity C is varied until the entire load, including both the lines and local

FIG. 14.



circuit, is in resonance, as indicated by a maximum reading of the line ammeter. In this condition the reactance of the capacity C exactly neutralizes the combined reactance of the inductance L_c and of the lines; readings of the series capacity and frequency are noted. Switch S is then thrown into position II, which in effect short-circuits the lines so far as the generator is concerned; maintaining the frequency the same as before, the series capacity is again varied until resonance is obtained and a second value noted. The algebraic difference in values of the reactance of the series capacity for the two settings gives the sign and magnitude of line reactance. The line resistance was measured by the " half-

deflection" method. The theory of the reactance measurements is briefly as follows:

For a given frequency, $f = \frac{V}{\lambda} = \frac{\omega}{2\pi}$, where V is the velocity of light, let

X_L = reactance of the line load, (positive or negative),

C_1 = value of capacity C, which gives resonance when switch S is in position I

$X_1 = \frac{1}{\omega C_1}$ resonant reactance of C_1 viz., of lines, plus inductance, L_e, plus leads.

C_{II} = value of capacity C which gives resonance when switch S is in position II. i.e., lines short circuited.

$X_{II} = \frac{1}{\omega C_{II}}$ resonant reaction of C_{II} viz., of inductance L_e and leads.

$X_e = \omega L_e$, reactance of inductance L_e, plus leads.

$\triangle R_1$ = resistance of line load, plus inductance, plus leads.

$\triangle R_2$ = resistance of inductance L_e plus leads.

R_L = resistance of lines.

Then at resonance, with $\triangle R_1 = O$, and switch S in position I,

(5).....$X_1 = X_L + X_e$

With S in position II, also at resonance,

(6).....$X_{II} = X_e$

Therefore subtracting (6) from (5),

$$X_L = X_1 - X_{II}$$

(7)..... $= \frac{1}{\omega C_1} - \frac{1}{\omega C_{II}}$

If $X_1 > X_{II}$, X_L will be positive, if $X_1 < X_{II}$, X_L will be negative. Readings of λ, C_1 and C_{II} therefore permit of the calculation of the effective line reactance. The resistance was measured by the *half-deflection method* which at the two resonant points, yields,

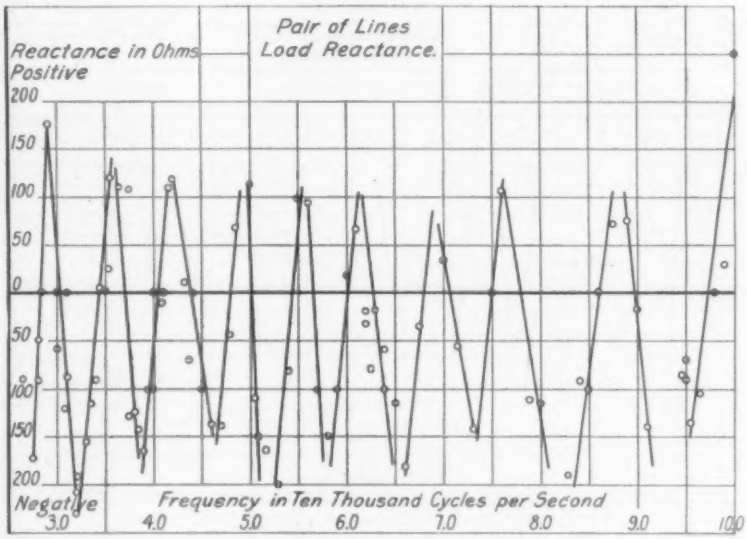
$$R_L = \triangle R_1 - \triangle R_{II}$$

The results of the measurements are shown graphically in Figs. 15, 16, 17, 18, 19 and 20. In Figs. 15 and 16, respectively, are plotted values of the sending end reactance and resistance of the pair of lines employed, as functions of the frequency, for frequencies between 25,000 and 100,000 cycles per second; in Figs. 17 and 18, respectively, are plotted values of the same two

functions in terms of the wave-length, for the range of wave-lengths 3000 to 300 metres (frequency 100,000 to 1,000,000 cycles per second). In Figs. 19 and 20, the variation of the sending end reactance and resistance of one line with ground return, as a function of the wave-length is plotted for wave-lengths between 3000 and 300 metres.

In making the reactance and resistance measurements, difficulty was experienced in obtaining the necessary accuracy re-

FIG. 15.

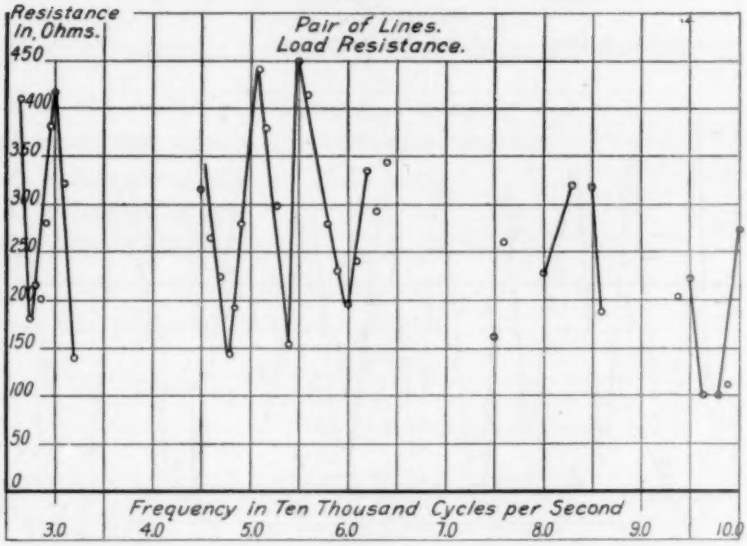


quired at the low frequencies (long wave-lengths). At times a change in frequency, which could hardly be detected by the wave-metre, was sufficient to cause the line reactance to vary from relatively high positive to equally high negative values. The physical conditions of the lines also seemed to vary at times during the measurement, causing the resonant point to rapidly shift back and forth with a consequent fluctuation in the ammeter reading. The points plotted in Figs. 15 and 16 were obtained with all possible accuracy and the curves drawn there through qualitatively confirm the prediction of the theory. The errors of observation in the wave-length values, from which the frequencies

were computed, are magnified by the abscissa scale chosen for these figures which permits of plotting to a greater accuracy than could be obtained in making the readings; this particular choice of scale was made in order to secure greater ease in plotting.

In accordance with theory as shown by equations (3) and (4), if the line resistance and leakage conductance are negligibly small, for certain frequencies, the reactance should attain infinitely large values. A glance at Fig. 15 shows this not to be the case for the particular lines measured; so far as could be

FIG. 16.



determined the reactance not only did not reach excessively high values, but passed through a second set of zero values. The actual measured value of the sending end resistance further shows that the resistance factor, which represents the combined effect of the linear resistance and leakage conductance, is not a negligible quantity. It will be observed on comparison of Figs. 15 and 16, that at zero reactance points where the slope of the reactance curve is positive, where data was obtained, the resistance reaches its minimum value; maximum resistance values are obtained also at zero reactance points, but where the slope of the reactance curves is negative; the average value about which the resistance varies is

approximately 250 ohms. Points of zero reactance occur at intervals of approximately 6500 cycles from 28,000 to 75,000 cycles per second, beyond which up to 100,000 cycles per second, they

FIG. 17.

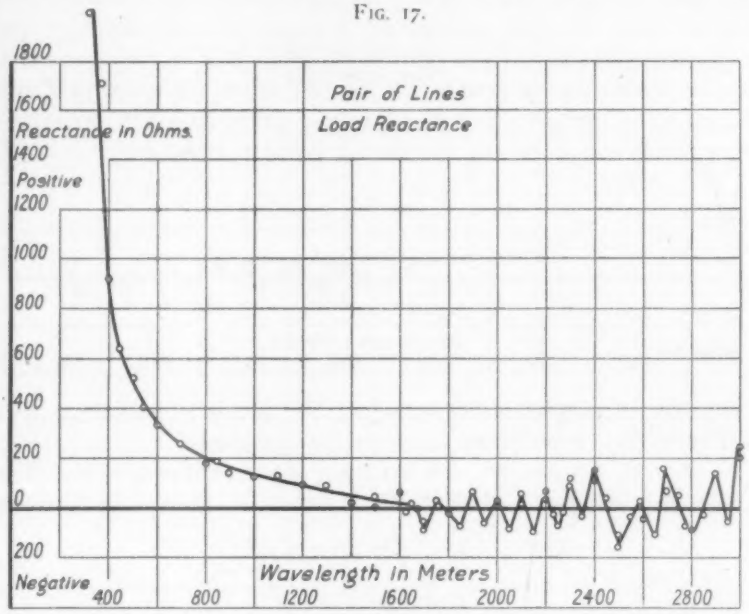
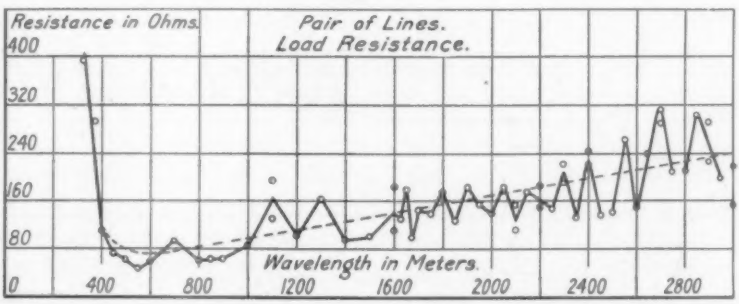


FIG. 18.

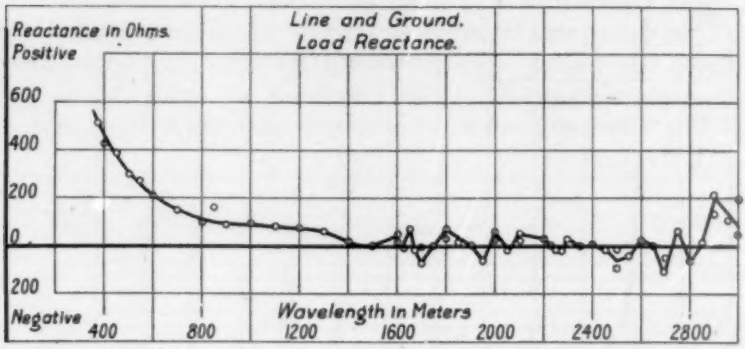


occur at intervals of nearly 11,000 cycles per second. The curves in general are similar to those obtained by Morecraft[17] for an artificial line at low frequencies. The reactance and resistance measurements were made with the lines in ordinary telephone and

[17] Morecroft: See previous reference.

telegraph operation as these are the conditions under which wired radio apparatus will be forced to work. The exact qualitative and quantitative confirmation of the theory is hardly to be expected

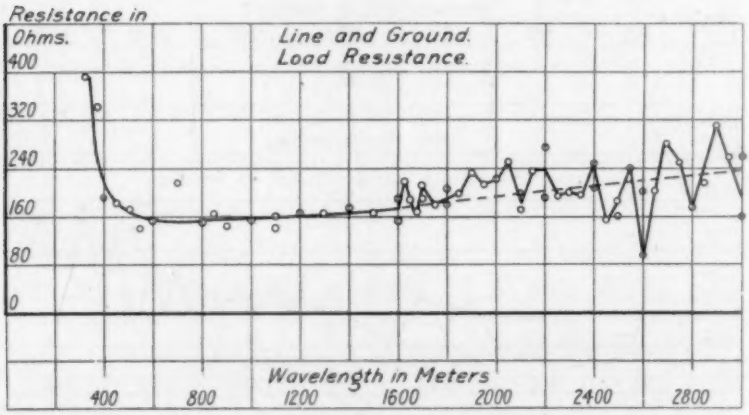
FIG. 19.



since the line conditions under which the measurements were made differ widely from those assumed by the theory.

In Figs. 17 and 18, for accuracy and convenience, the reactance and resistance of the pair of lines, are plotted in terms of

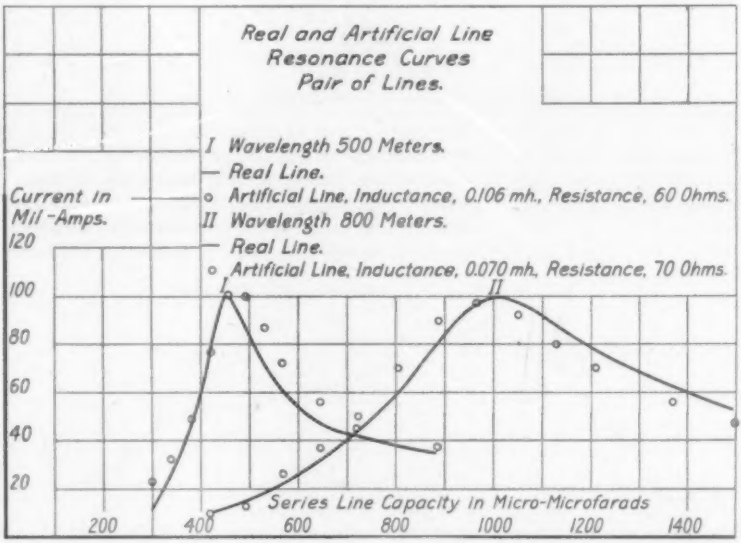
FIG. 20.



the wave-length, for wave-lengths between 3000 and 300 metres (frequency 100,000 to 1,000,000 cycles per second). Figs. 19 and 20 show the same quantities for one line and ground plotted in the same manner. From Figs. 17 and 19 it is seen that the periodic variation in reactance persists, as at the lower frequen-

cies, but not in so definite manner, until a wave-length of 1600 metres (187,500 cycles per second) is reached, below which it remains positive in sign, but increases in a quite uniform manner; the pulsation in resistance values for both line and line, and line and ground, also continues as shown in Figs. 18 and 20, the average value in both cases decreasing until a wave-length again in the proximity of 1600 metres is reached, below which, similar to the reactance, it increases, more or less uniformly, at a very rapid rate, and in a manner similar to the resistance of a radio

FIG. 21.



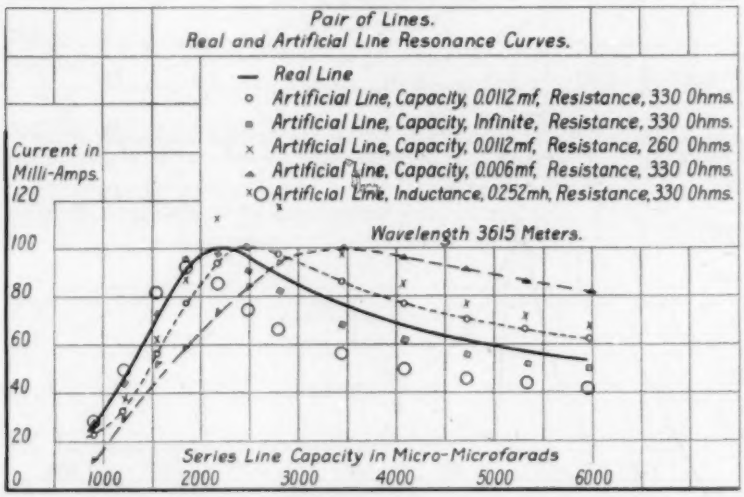
antenna. The resistance of the pair of lines at the very short waves averages about one-half of that of one line with ground return. This sudden transition in the properties of the line load is unique and is not to be entirely accounted for by the theory. The preponderance of the resistance of the line and ground load over that of a pair of lines, which manifests itself at the higher frequencies may be attributed to the more pronounced radiating properties possessed by the former type of load, at these frequencies. At the lower frequencies, the radiating properties of both systems become greatly reduced.

To check the accuracy of these curves and to test their

validity when applied to actual practical cases, a number of artificial line loads were built up consisting of values of inductance, capacity and resistance, as nearly as possible equal to those as specified by the curves. Values of line current as a function of the series line capacity, were obtained, first for the real line load and then for the artificial line load of the constants specified. This data, plotted in the form of resonance curves for different frequencies, is shown in Figs. 21, 22, and 23.

In these figures the heavy line curves represent the current variation obtained for the real line load, and the small circle points represent the current variation obtained for the artificial

FIG. 22.

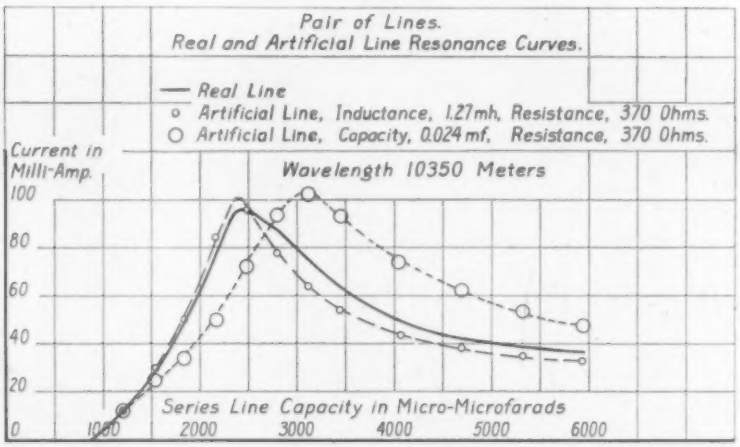


load of the proper constants; the cross, square, triangle and large circle points represent the current variation obtained with artificial loads of characteristics differing from the specified ones by the amounts indicated. The agreement between real line values and those obtained on the proper artificial load is observed to be unusually exact. Any great departure from the conditions simulating the real line load causes a shift in the resonance point as well as an alteration in the shape of the resonance curve.

Throughout this investigation special interest, of course, has been attached to the behavior of the line load at the high range of radio frequencies, *viz.*, 1,000,000 to 200,000 cycles per second

(300 to 1500 metres), since it is within this range that the transmission and reception tests were conducted. While from the data obtained, it is always possible to construct artificial loads which will simulate the real line load in its characteristics for individual frequencies, it is both desirable and convenient, from an experimental standpoint, to build up an artificial load which will simulate at least the reactive component of the real load over a given range of frequencies. The variation in resistance with the frequency may be taken care of by a properly constructed variable resistance. From the reactance curves of Figs. 17 and

FIG. 23.

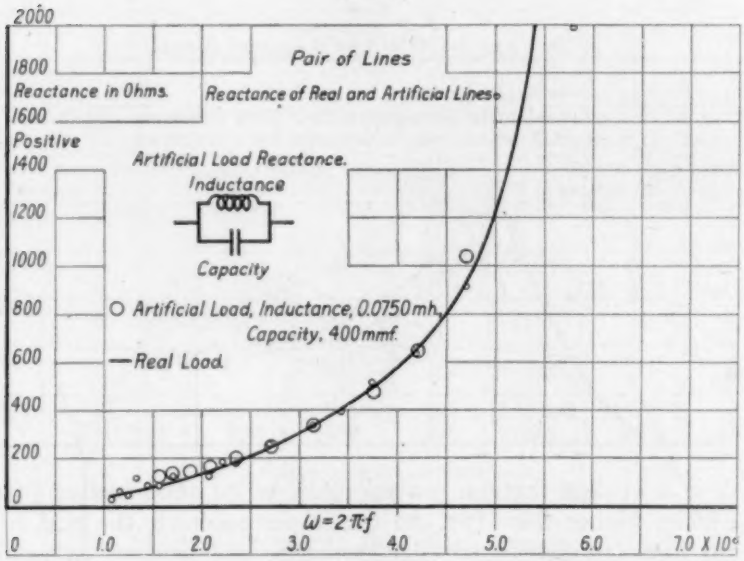


19, it is evident that for wave-lengths below 1600 metres (frequencies higher than 187,500 cycles per second), the real line load loses its distributed characteristics, and behaves as would a circuit of lumped constants. The reactance curves of the line load which obtain for the short wave-lengths are similar in shape to that of a lumped inductance, operating near its fundamental wave-length where the effect of the distributed coil capacity is appreciable. This leads to the belief that by properly choosing a value of inductance, which it is assumed to be free from capacity, and a parallel connected capacity, it should be possible to build up a circuit which would closely simulate the real line load in its reactive properties. For purposes of experimentation, over frequencies from 900,000 to 200,000 cycles per second (wave-

lengths 333 to 1500 metres), an artificial load has been constructed which represents the real line load of this range of frequencies.

By a method outlined by Kolster,[18] it was computed that a *true* inductance of 0.0789 milli-henrys in parallel with a capacity of 350 micro-microfarads, would give the desired reactance variation. Without resorting to extreme precision in construction, an inductance was built whose value was computed to be 0.07660 milli-henrys, and whose actual value, measured both at 1000 cycles and over the operating range of high frequencies extending

FIG. 24.



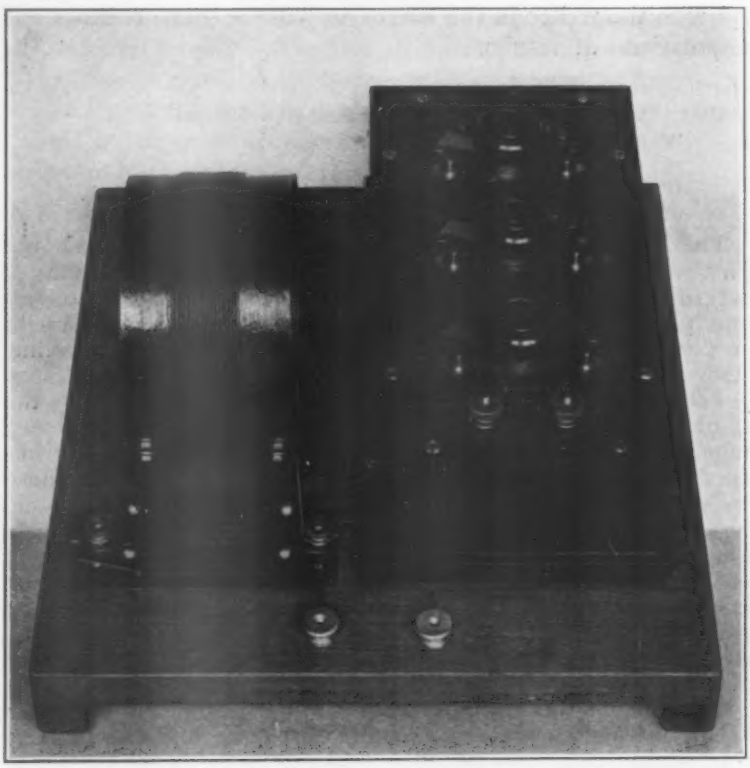
up to 1,000,000 cycles per second, was 0.0750 milli-henrys. This inductance in parallel with a capacity of 400 micro-microfarads gives the measured reactance variation as shown by the large circle points in Fig. 24; the full line curve of this figure represents the reactance of variation obtained for the pair of lines plotted as functions of ω. The agreement between the reactance values of the real and artificial loads is seen to be quite exact. A view of an artificial load is shown in Fig. 25.

[18] Kolster: *Proc. I. R. E.*, Vol. 1, No. 1, p. 19, 1913.

VII. ACKNOWLEDGMENTS.

In conclusion, it is desired to acknowledge the active support and assistance rendered by a number of persons throughout the course of these experiments. To Major-General George O. Squier, Chief Signal Officer of the Army, original credit is, of course, given; the experiments described herein are a continuation and amplification of General Squier's original work. The hearty coöperation of the officials of the Pennsylvania and Boston and Albany Railroads, and of the New York Central Lines, and in particular of Mr. E. C. Keenan, General Superintendent of Telegraphs of the latter system, in the furnishing and movement of the two railroad coaches, and in placing the telephone lines at the disposal of the Signal Corps, has of course made the later

FIG. 25.



experiments possible; to Mr. Keenan's initiative, the commencement of the later experiments is due. In the Signal Corps party it is desired to mention the services of the enlisted personnel, consisting of Sergeants first-class Charles Murray, Fred Brennen, and W. L. Redmond. It is also desired to mention the services of Assistant Radio Engineer Samuel Isler, who throughout these experiments was in charge of the fixed station, and who coöperated with the writer in the line of impedance measurements and computations of results.

OFFICE OF THE CHIEF SIGNAL OFFICER,
WAR DEPARTMENT, ENGINEERING AND RESEARCH DIVISION,
WASHINGTON, D. C.,
October 4, 1920.

The Mass-spectra of Chemical Elements. F. W. ASHTON. (*Phil. Mag.*, November, 1920.)—The positive-ray method of analysis is extended in this paper to the elements fluorine, sulphur, phosphorus and arsenic, which appear to be made up of one kind of atom only, and also to boron, silicon and bromine, which are found to be composed of two or three isotopes. Since the examination of these seven additional elements exhausts the list of those which can be investigated as gases or as gaseous compounds it is pertinent to scrutinize the results thus far obtained. In all eighteen elements have been examined. Of these nine seem to be simple, *i.e.*, to consist of one type of atom alone. These with their atomic weights and the masses of their single isotopes as determined experimentally are as follows: Hydrogen, 1.008 (1.008); helium, 3.99 (4); carbon, 12.00 (12); nitrogen, 14.01 (14); oxygen, 16.00 (16); fluorine, 19.00 (19); phosphorus, 31.04 (31); sulphur, 32.06 (32); arsenic, 74.96 (75). It is to be noted that the ordinary atomic weights of all these are either exact integers or very nearly so. The remaining nine elements with their atomic weights and the masses of their isotopes in the order of their intensity are boron, 10.9 (11, 10); neon, 20.20 (20, 22, 21?); silicon, 28.3 (28, 29, 30?); chlorine, 35.46 (35, 37, 39?); argon, 39.88 (40, 36?); bromine, 79.92 (79, 81); krypton, 82.92 (84, 86, 82, 83, 80, 78); xenon, 130.2 (128, 131, 130, 133, 135, all somewhat uncertain); mercury, 200.6 (197–200?, 202, 204). The atomic weights of these depart from whole numbers by larger quantities than is the case in the previous group of elements.

Is it too much to expect that methods will be devised for the separation of the two isotopes of boron with atomic weights of 11 and 12, respectively?

G. F. S.

AËRONAUTIC INSTRUMENTS.*

BY

CHARLES E. MENDENHALL, PH.D.

Professor of Physics, University of Wisconsin.

THE number of instruments which are of use in aircraft is rather surprisingly large, and while this article must be limited to the consideration of a few of them it will nevertheless be instructive to give a fairly complete list in which the instruments are arranged in groups in accordance with their primary purpose, but not in the order of their importance. The method of mounting some of these instruments is shown in Fig. 1.

A. INSTRUMENTS FOR SHOWING CONDITION AND PERFORMANCE OF POWER PLANT.

1. Tachometer.
2. Radiator thermometer.
3. Oil pressure gauge.
4. Air pressure gauge for gasoline system.
5. Gasoline level or supply indicator.

B. INSTRUMENTS FOR INDICATING OR MEASURING AIRPLANE PERFORMANCE.

6. Altimeter.
7. Airspeed meter. } Also used for navigation.
8. Angle of incidence meter.
9. Side slip indicator.
10. Rate of climb meter—statoscope.
11. Bubble or pendulum inclinometer.
12. Gyroscopic inclinometer.
13. Accelerometer.

C. NAVIGATIONAL INSTRUMENTS.

14. Compass.
15. Turn indicator.
16. Watch.
17. Sextant.
18. Drift meter.
19. Bearing plate, etc.
20. Ground speed meter.
21. Stabilizing devices.
22. Wireless equipment.
23. Position indicator.
24. Night landing devices.
25. Oxygen apparatus.

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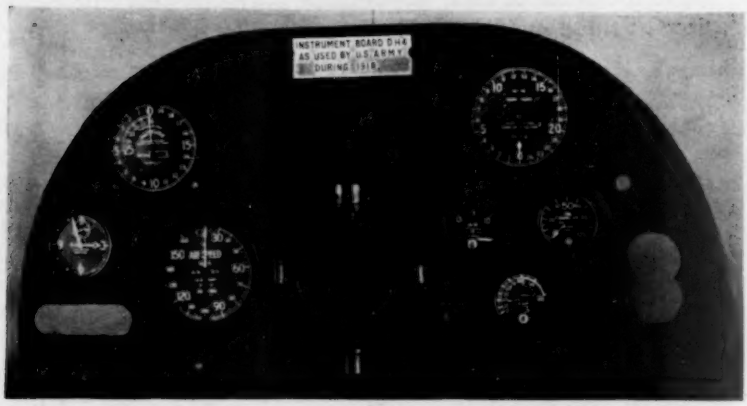
* Presented at a meeting of the Section of Physics and Chemistry and the Physics Club held Thursday April 1, 1920.

D. INSTRUMENTS FOR MILITARY PURPOSES.

26. Bombsights.
27. Gunsights.
28. Cameras.

It should be pointed out in the beginning that there are several especially adverse conditions accompanying the use of instruments on airplanes, which for some instruments become very serious indeed. These are excessive vibration, great and rapid change of temperature, large accelerations, and the necessity for keeping

FIG. 1.



down weight and bulk. Other difficulties of a less general nature will be referred to from time to time.

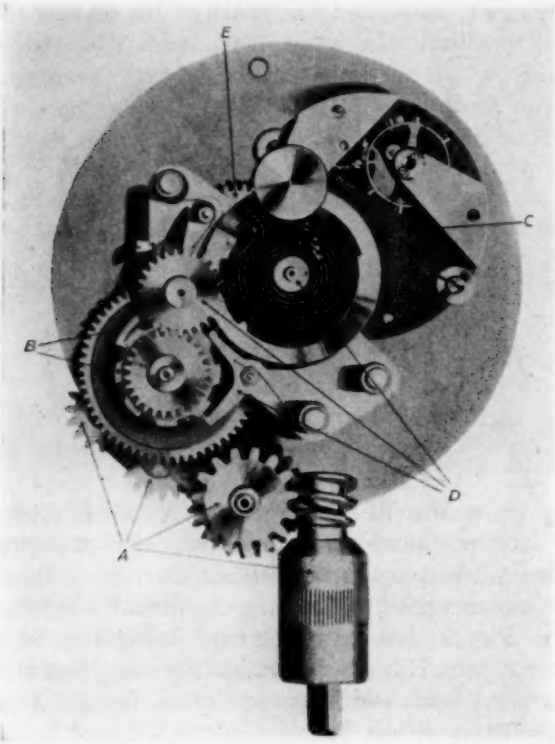
Tachometer.—The most important engine instrument is the tachometer, because it first gives warning of improper functioning, and the most successful have been either of the chronometric or the centrifugal type. The former, which has been developed largely during the war, involves two distinct but interacting elements, *i.e.*, a time-measuring device or watch mechanism containing a spring-driven escapement, and a counting mechanism containing a hand or pointer moved by the drive from the engine through either a circular or linear rack. The hand is turned at a speed proportional to the engine speed for a fixed interval of time determined by the escapement. Hence the angle through which the hand is turned will be proportional to the engine speed. At the end of this motion the hand is locked in position, while the

driving mechanism returns to the initial position and the cycle starts over again. If the engine speed is increasing the mechanism operating the hand will be turned farther during the next driving interval, and at the end of this interval the hand will be pushed ahead and again locked. If the speed is decreasing the hand will drop back, while if the speed is constant the hand will be again locked in the original position. Thus the cycle of operations consists of a driving interval, an interval during which the hand is adjusted to the new position of the driving mechanism, the locking of the hand, and the return of the driving mechanism to the zero position. In some instruments this complete cycle takes one second, in others two, the hand being reset each second or every two seconds as the case may be. The number of teeth on the rack of the driving mechanism which controls the position of the hand obviously determines the smallest interval to which the hand can respond as the locking arm must engage one tooth or the next, but this can be easily made less than 5 per cent. of the normal reading. The chief inherent difficulties of this type may be said to be liability to wear and breakage due to multiplicity of parts, many of which have to be continually started and stopped, and a tendency of the hand to jump back and forth through the interval determined by a rack tooth.

In 1917 there were two tachometers of this type in use abroad, the "TEL" having a relatively heavy and substantial mechanism, and the "JAEGER" built more like a clock or watch. Operating under the TEL patents the National Cash Register Company later produced many thousands of instruments for the Air Service, while a new and excellent instrument was designed by Mr. Prouty and produced in large numbers by the Van Sicklen Company. Fig. 2 shows the internal mechanism of the latter form. In 1918 the Hamilton Corporation completed the development of a third form which has one novel feature, namely, that the instrument is driven by air pulses produced by a compact reciprocating pump attached to the engine and transmitted through stiff-walled rubber tubing about $\frac{5}{8}''$ outside diameter. This question of drive is of particular importance in installations on large machines, in which it may take 35 feet or more of the usual flexible shafting, used with all mechanically driven tachometers, to connect the engine with the indicating head on the instrument board. Such a length of flexible shaft, especially

when very much bent, as will unavoidably be the case, has the disadvantage of absorbing very considerable power, of wearing rapidly, and of providing opportunity for the core or shaft proper to pull away from the end connections (which must be slip connections) unless the length of core is adjusted for the bends of each particular installation. For such cases the air-driven type is of considerable promise.

FIG. 2.



The centrifugal type of tachometer, the more common in ordinary use, has also been largely used for air work, both in this country and abroad, a number of types being developed for this purpose, some of most excellent performance. There may be mentioned the Jones, Reliance and Johns-Manville. An instrument of this type was developed in 1918 by Doctor Washburn of

the Bureau of Standards, as the result of extended tests, but did not reach the production stage.

The chief inherent difficulties with tachometers in general are perhaps those of wear and lag (lost motion), both of which demand for their reduction good design, good workmanship and good materials. At first sight it might appear that the centrifugal type would give more nearly instantaneous readings than the chronometric, which responds only every one or two seconds, but such is not the case. Since its moving system has considerable inertia and rotates at high speed it requires an appreciable time to adjust itself. While this is a disadvantage in one way, it smooths over small fluctuations in speed and gives a steadier hand.

The very common magnetic type of tachometer has not been satisfactorily developed for air use; nor has the electric, though the latter offers some advantages in the case of long transmissions on large planes. Several air-viscosity types, and an airflow type were experimented with, but never brought to a production stage.

Radiator Thermometer.—The conditions of use here demand a distant-reading instrument, and two types are used. One involves a "bulb," containing a liquid whose saturated vapor pressure is transmitted through a tube to an indicating head or pressure gauge; the other a "bulb," connecting tube and pressure head, all filled with a liquid, whose relative expansion produces the pressure chang which is measured. For the former type ethyl ether, sulphur dioxide, or methyl chloride, may be used, for the latter alcohol is commonly employed. In both cases temperature compensation of the pressure head is needed, while for the vapor pressure type the volumes of the bulb, tube, pressure head, and the amount of liquid must be so proportioned that the *meniscus* is always in the bulb, and hence is at the temperature to be measured. If there is too little liquid it may entirely evaporate from the bulb when the latter is the hottest part of the system, and if there is too much it may more than fill the bulb if this is the coldest part of the system, the meniscus being somewhere in the tube.

With the liquid-filled type, it is important that the volume of the tube and instrument should be small compared to that of the bulb, in order to reduce errors due to changes in volume of the former. Both types are subject to an error due to change in

barometric pressure with height or otherwise, which is of course smaller the higher the internal pressures which are used. Thus an ether vapor pressure thermometer will at 20,000 feet indicate about 3° high, but this is not a serious matter; with a liquid-filled thermometer this error may be very much less. Since the purpose of the thermometer is to warn of overheating of the engine, and possible freezing of the cooling water, no great accuracy is required.

Oil and Air-pressure Gauges.—Since there is usually a forced circulation of lubricating oil, a simple pressure gauge will suffice to show whether the oil pump is functioning properly and the circulating pipes free, and the oil not too cold and viscous to flow properly. If, as is still commonly the case, the gasoline is fed by air pressure, a simple air-pressure gauge is provided to indicate when a proper air pressure is being maintained above the gasoline in the fuel tank. For military use, however, the pressure feed system was being displaced as rapidly as possible, because of the inherent danger: a puncture resulting in a particularly vigorous leak on account of the five or ten pounds' pressure which was used.

Gasoline Level Indicators.—A similar argument of increased danger applies against a certain type of gasoline level indicator which appeals to a physicist because of the simple application of hydrostatic principles. In this case a pressure gauge is connected to a tube extending to the bottom of the fuel tank; through this tube air is forced by a hand or power pump. Obviously the maximum air pressure which can be obtained is that necessary to force the air out against the pressure of the gasoline at the bottom of the tank. This pressure is of course proportional to the depth of gasoline, and the pressure gauge can be graduated to read inches of depth. By making the case of the instrument air-tight and connecting it to the top of the tank, the scheme may be adapted to a pressure feed system. Though this gauge was made and used to a certain extent in France, it has never been adopted in this country. The usual gauge is of the float type, and there are two rather troublesome problems involved: first, the design of a simple rugged float system which will not stick or jam, and second, the transmission of the float motion, as is in some cases necessary, some considerable distance to the indicating head. When the latter is directly in the tank the problem

is much simpler, and there are doubtless many mechanisms which will work, but it is equally true that many more have been designed which will not work. At least two fairly satisfactory ones were developed during the war—the Tanner and the Cole—the latter having a transmission system copied from an old-fashioned house bell; a similar transmission mechanism was also used by the Germans.

Altimeter.—This is of course merely the familiar aneroid barometer, the essential features (see Fig. 3) being a more or less perfectly exhausted flat metal box with corrugated elastic

FIG. 3.

Mechanism of altimeter.

sides, one of which is rigidly attached to the case, the other moving as the external pressure changes. Various types of mechanism are used to magnify this motion, the ultimate result in our standard instruments being a complete rotation of the pointer for a change of pressure of about 4 cm. of mercury, corresponding to a height of 20,000 feet, with a uniform scale.

Since atmospheric pressure does not vary in simple inverse relation to altitude, the production of a uniform scale demands a specially designed mechanism involving either a cam, or more commonly levers, whose effective arms vary from point to point on the scale. The difficulties are of two sorts: First, those of the instrument considered as a pressure measuring device, and second,

those involved in the relation between pressure and height. Of the first class may be mentioned elastic after-effect, "sticking," and temperature error, of which the first is the most difficult to reduce. The restoring force which acts in opposition to the external pressure on the cell is due in part to the elastic forces of the cell itself and in part to an external or internal spring, which may be of the best quality of steel. The cell is usually made of nickel-silver, more subject to elastic after-effect; hence it is desirable to proportion the cell so that as much of the restoring force as possible may be due to the spring. A systematic study of the elastic properties of alloys suitable for cell construction, as a function of the composition and method of manufacture (heat treatment) was undertaken by the Bureau of Standards and the Air Service coöperatively, and has been continued by the former. The results of this may lead to considerable improvement.

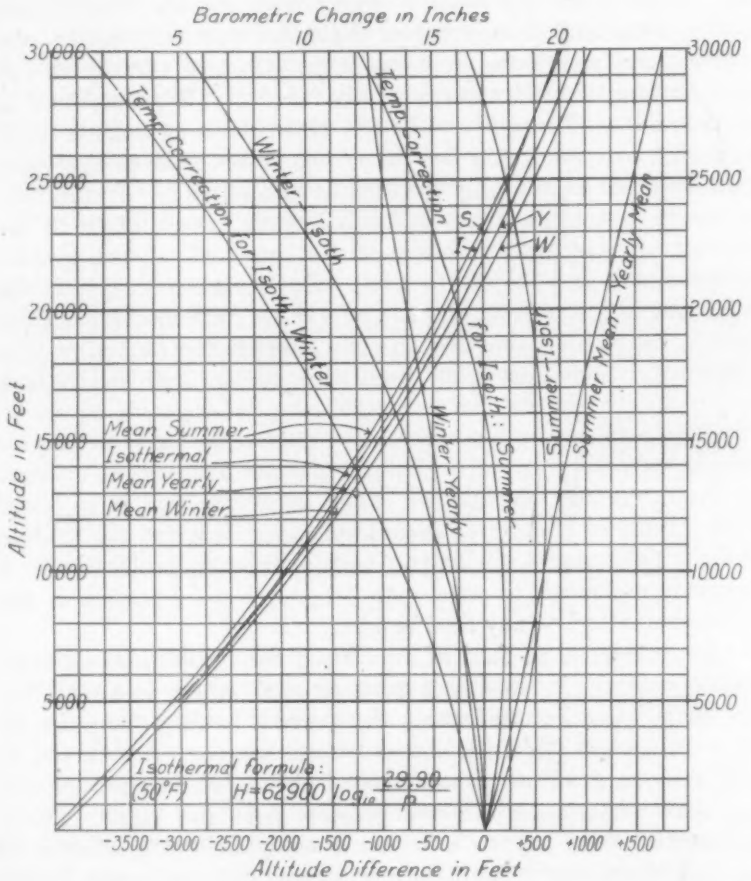
The avoidance of "sticking" of the mechanism depends largely upon good workmanship and simplicity of design, while temperature compensation may be partly attained by imperfect exhaustion of the cell and by the introduction of an element into the mechanical train whose thermal expansion compensates the thermal-elastic changes of the spring and cell.

Granted the attainment of a satisfactory pressure-measuring instrument, there remains the calibration of it to indicate height. This is now universally carried out by means of the equation $H = 62900 \log_{10} \frac{29.90}{P}$ in which H = height in feet and P = atmospheric pressure in inches of mercury. This equation is based on the assumption of an isothermal atmosphere at 10° C., and the corresponding average humidity, and neglects the small variation of "g" with height. The assumption of an isothermal atmosphere is of course erroneous, and it is worth while considering the magnitude of the errors involved and the possibility of allowing for them, as well as the question whether a better basis for calibration could not be used.

The diagram (Fig. 4), based on computations of Lieutenant Waterman of the army Meteorological Service, using data published in the *Monthly Weather Review*, January, 1918, by W. R. Cregg, shows the magnitude of the errors to be expected on the basis of the observed average summer, winter and yearly tem-

peratures, with an instrument graduated on the usual isothermal basis. One may conclude from Fig. 4 and similar curves for spring and fall, that the error in fall, winter and spring would be very considerably less if altimeters were graduated in accord-

FIG. 4.



ance with the yearly mean temperature distribution, while the errors in summer would be increased at high altitudes.

Aside from the *average* errors to which Fig. 4 refers, there are errors due to the departure of the temperature distribution from that assumed in calibration, whether this be the isothermal, as usually assumed, or the yearly or summer mean. Such varia-

tions from the mean are very common at lower altitudes, but are not likely to extend as high as 10,000 ft. Lieutenant Waterman concludes that the maximum error of this sort is perhaps −500 or −600 ft. at 3000 ft.

The only way of reducing the very considerable[1] temperature errors which are likely to occur with any fixed calibration (unless one used three separate dials, for summer, winter, and spring and fall) is by applying a temperature correction. If observations of air temperature are taken at the ground and at the maximum elevation, and better, at several intermediate levels as well, a correction may be taken from tables, such as have been published, for example, by the Bureau of Standards. In this way it is possible to reduce the outstanding error, as estimated by the Bureau of Standards, to one-half of 1 per cent. Since ground temperatures are subject to great local irregularities, it is the usual practice to observe at 5000 ft. and use the empirical relation $t_g = t_{5000} + 7^\circ$ C., which is accurate enough for moderate temperatures. Some work has been done in England and is now being initiated in this country, upon the development of an instrument which would automatically correct to some extent for the main temperature error, by utilizing the temperature changes in the instrument during the climb, assuming that it took the temperature of the air at each level, or at least at the ground and the final height. This is a very interesting problem, and it would seem possible to make some progress toward a solution, but I have not heard of any final results.

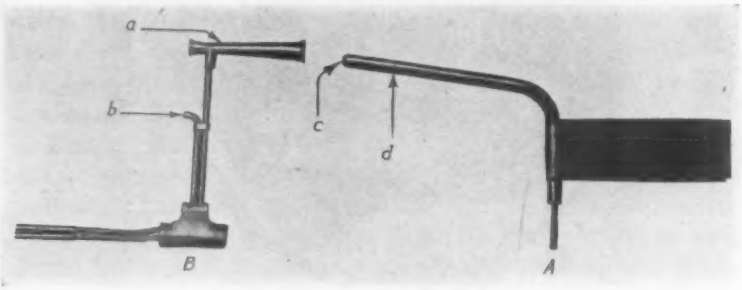
The universal method of correcting for fluctuations in barometric pressure by having a rotating dial, which is always set to "zero" just before leaving the ground, is only approximate, since in general barometric fluctuations are not proportional to the pressure gradient at successive heights, but is good enough for daily changes, which are greatest near the earth. For a permanent shift, say to a higher landing field, the scheme of shifting the zero of a uniformly graduated dial makes the desired correction at all heights, in so far as the calibration of the instrument is itself correct.

[1] It is obvious that the careful application of the temperature corrections is necessary in deciding as to all altitude "records." For a discussion of this aspect of the recent flights of Messrs. Rohlfs and Schroeder see *Science*, April 9, 1920.

Obviously the altimeter, because of inaccuracy and lag, is of no use in landing, except in a general way, but it gives an essential element in the determination of ground speed, for use in bomb-dropping, and as our knowledge of upper air currents increases, it will be used in locating favorable wind-levels, especially when flying above clouds.

Airspeed Meter.—The relative motion of an airplane with respect to the air is an essential factor in determining its stability, and the airspeed meter is used to determine the component of this motion parallel to the axis of the machine. This statement is really inaccurate and will now be corrected, for which purpose a digression is necessary. The motion of the plane through the air develops a complicated set of forces of which those on the

FIG. 5.

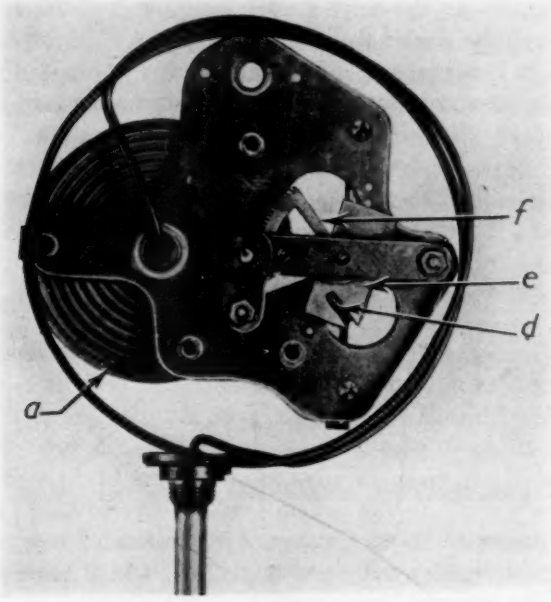


A, pitot tube, Sperry type; *B*, Venturi-pitot tube, army type. (Bristol Co.)

struts, wires, side surfaces, etc., are in the line of motion, and of the nature of a resistance to motion (R), which varies approximately as the square of the speed, S, and as the density of the air. Besides these there are the forces on the wings and certain portions of the fusilage which, besides having resistance components parallel to the motion, also have large components perpendicular to this, the resultant of which is called the "lift" L, which also varies approximately as S^2d, where d is the density of the air. The remaining forces acting are the weight W, and the thrust of the propeller T. For certain values of W, T, R, and L, the plane will be capable of stable motion with uniform velocity. Since R and L vary approximately as S^2d, we may conclude that, assuming W and T fixed, corresponding conditions of motion at different altitudes will correspond to fixed values of S^2d.

Hence, it is desirable to have an instrument which gives, not a given reading for the same relative air speed at all heights, but one which gives the same reading for the same value of S^2d, at all heights. The actual air speed would then be proportional to observed speed over d. The particular conditions of motion which are of special importance to the aviator are: the best condition for climbing and gliding, most economical speed

FIG. 6.



Mechanism of air speed meter (Bristol type).

for level flying, and the stalling conditions, though the latter can be well enough determined by the "feel" of the ship.

For assistance in handling the plane, since true air speed is not desired, the most common instrument is either a pitot or a venturi tube, or the two combined, as shown in Fig. 5. These must be combined with an instrument for measuring pressure differences (see Fig. 6), consisting of a mechanical magnifying system and some sort of diaphragm; with the pitot tube, one side of this diaphragm is exposed to the "static" pressure, the other to the "impact" pressure, by air-tight tubes running from the

"head" to the indicating instrument. In the case of the pitot-tube, theory (which is admittedly only fairly satisfactory) indicates that the pressure difference produced will be equal to S^2d, and this has been confirmed as regards the S^2 factor over a range of moderate speeds by observations with a whirling arm at the National Physical Laboratory, London. In fact, the pitot tube is at present the basic absolute standard for practically all airspeed measurements, both in the open and in wind tunnels. For high speeds (over 130 m.p.h.) a correction is required according to theory because of the compressibility of the air (see Buckingham 1st Rep. Am. Nat. Adv. Com. for Aeronautics), but further experimental research in this connection is desirable. The venturi tube produces a suction which, if the flow through the tube were non-turbulent and the compressibility of the air were negligible, would, according to Bernoulli's theorem, also satisfy the relation of $p_1 - p = KdS^2$. Unfortunately, neither of the above assumptions hold, so that the "theory" of the venturi tube is of no practical value except to indicate that probably the variation in proportion to d may be somewhere near the truth. The Bureau of Standards has recently obtained experimental evidence on this point. It is possible to design a venturi which will satisfy the S^2 law for a certain range of speed as was first done by Doctor Zahm. This was the first tube adopted by the army. Later, Lieutenant Oliver and the writer, working at the Bureau of Standards, designed another tube which followed this law through a still wider range (up to 150 m.p.h.) and was only of one-half the size and had less than one-quarter the head resistance. This and the English standard Pitot are shown in Fig. 5, where a is the Venturi tube and b the corresponding impact opening, c the Pitot and d the static openings.

The pitot tube has the advantage of simplicity and ease of manufacture, since almost any sort of a forward-pointing tube will do. On the other hand, quantity production of the venturi-pitot combination, shown in Fig. 5, was found easily practicable, and since it can be made to give at least five times the pressure difference at any given speed, a more rugged pressure measuring instrument may be used. It was for this reason that the venturi-pitot was adopted by our Air Service. For seaplane use it has the additional advantage of being less liable to plugging by splashing water.

In another form of airspeed indicator a plate is exposed normally to the air stream, held in position by a spring whose compression, due to the impact pressure, is indicated in a dial. If provided with a sufficient guard plate, at the cost of increased head resistance, such a plate will, as Zahm showed, give pressures agreeing with a pitot tube. Without a guard ring the agreement will be only approximate. Another form of instrument which must be mentioned consists of some form of screw or cup anemometer, whose speed of rotation may be indicated by some form of tachometer attached either directly, or else through electrical or mechanical transmission to the cockpit. This gives true and not effective air speed, and hence is not so convenient from the standpoint of handling the ship. On the other hand, for use in dead reckoning and for bomb dropping its readings are what is wanted, while the readings of the other type of instrument must be corrected for altitude.

It should be emphasized that the location on the plane of the venturi or pitot tube, or plate or screw, is of great importance. The airflow is so much disturbed in the neighborhood of the wings that it is usually impossible to observe correct air speeds unless the tube or head is held several feet in advance of the leading wing and well away from the screw and fusilage. Such a position being impracticable in general on account of the increased head resistance of the mount, a " plane correction " should be determined for each type of plane and position of mount by speed-course trials. As regards use in handling the plane, absolute values are not needed, constancy of calibration being more important.

Angle of Attack Meter.—The angle of attack is the angle between a wing chord and the line of flight. For horizontal flight there is just one air speed possible for each angle of attack, hence the angle of attack has sometimes been used in place of air speed in handling the plane—notably by the Wrights. Since this angle is in general small, instruments have been designed for magnifying it and indicating it to the pilot, but have not come into much use. A string or tape whose end plays over a graduated arc is a simple but fairly effective device.

Side Slip Indicator.—One form of this consists of a small metal sphere provided with two small holes at the ends of the horizontal transverse diameter, which are connected (internally)

with the two sides of a sensitive differential pressure indicator. If there is a side slip, that is, if the relative air stream is *not* normal to the transverse diameter, a pressure difference of one sign or the other will be produced. If the holes are at the ends of a vertical diameter the instrument becomes a form of angle of attack indicator. In neither capacity is the instrument much used.

Statoscope.—The purpose of this instrument is to indicate when a plane is rising, falling or flying horizontally, and it is chiefly used in performance tests. The usual form consists of a thermos bottle, which can be quickly opened to or cut off from the external air by means of a stop-cock, and a scheme for indicating, or measuring, the pressure difference between the inside and outside, such as a U-tube pressure gauge containing oil. If the external pressures are equalized and the stop-cock shut at any height, then a rise or fall of the machine will be shown on the gauge as a decrease or increase of the external pressure. By this means the absolute height, so far as it is determined by constant external pressure, may be kept constant within 10 feet. A less sensitive form of instrument on this principle has been put on the market.

Rate of Climb Indicator.—If the above form of statoscope is provided with a capillary opening in place of the stop-cock, it becomes a rate of climb indicator, an absolute instrument in so far as atmospheric conditions are constant and normal. The approximate equation of the instrument is

$$\frac{d}{dt}(\Delta p) = K \Delta p P - \frac{P}{A}\frac{dH}{dt}$$

where

Δp = pressure difference between inside and outside of flask.
P = barometric pressure at height H.
K = constant depending on size, etc., of capillary opening.
H = altitude.
A = a constant in the assumed isothermal equation connecting altitude and pressure.

If Δp is maintained constant then

$$\Delta p = \frac{1}{AK}\frac{dH}{dt}$$

which will be independent of altitude. That is, a constant pressure difference corresponds to a constant rate of climb. Experiments of Captain Webster's showed that readings could be made

accurate to about 20 ft./min., with a capillary such that a new steady reading would be reached in about 5 or 10 seconds after a change in elevation had occurred.

Bubble or Pendulum Inclinometer.—In a properly-banked turn an airplane tips so that the resultant force compounded of gravity and the central acceleration, is normal to the floor of the plane, *i.e.*, bears the same relation to the plane that gravity does in straight horizontal flight. For a 60° bank, therefore, we have the relation that

$$Ac = \text{central acceleration} = \frac{S^2}{R} = g \tan 60^\circ = g\sqrt{3}$$

$$\text{and } g' = \text{apparent gravity} = g \sec 60^\circ = 2g$$

Any instrument, such as a level or pendulum, which responds quickly to gravity, will therefore remain in its symmetrical or zero position during a bank and be useless as an inclinometer. Either of these devices will show a deviation, however, when a plane is not properly banked and is therefore side-slipping, and hence are of some use as indicators of *incorrect banking.* As a matter of fact a measure of the angle of bank is of no general use, though an instrument which will show when a plane is turning, and therefore usually banking, is of great use and will be discussed later. Under certain circumstances a really effective inclinometer might be of use when it is desired to fly with as even a keel as possible, and for such use a variety of gyroscopic devices have been tried. The underlying principle of these will be taken up later, but one or two may be mentioned. The simplest, though in many ways the least satisfactory, is the gyroscopic top of Garnier. This is a heavy top supported very near the centre of mass on a steel pivot point, which rests in a steel or agate cup, mounted in a closed case with a spherical glass top centred at the pivot, and driven by air jets playing against the rim of the top. Strictly speaking, it is not a top, for the point of support is usually very slightly above the centre of mass. Speeds of 5000 to 10,000 r.p.m. may be obtained with an air pressure of only a few centimetres of mercury, and displacements of the top with respect to the case are read on graduations on the glass cover. It is a very pretty thing, but is too susceptible to the jars of ordinary service, and has besides several serious inherent troubles; its use has therefore not been found worth while.

Navigation of Aircraft.—The primary requisite in navigation is the ability to steer a straight course in a known direction; hence we may well introduce the subject by considering the two instruments which are designed to make this possible.

Compass.—The only practicable compass so far developed for air use is the magnetic (see Fig. 1 for standard army type), though there have been reports that a gyroscopic type will be made avail-

FIG. 7.

able. For various reasons the compass has been perhaps the most discussed and most abused instrument, chiefly because it has been expected to do two things: first, to give the direction of a maintained course; second, to indicate departures from this course. The first duty it can fairly well perform, but as we shall see, it is ill adapted to the second. A compass card of the usual type (see Fig. 7) is supported upon a pivot attached to the card, with the point slightly above the centre of mass, and has the equivalent of a counter-balancing weight on the south side to overcome

the couple due to the vertical component of the earth's magnetic field. In order to diminish the effect of vibration it is necessary to reduce as much as possible the pressure of the pivot on the supporting cup, and to do this and also introduce desired damping, the cards are immersed in a liquid, either alcohol or kerosene. The forces acting upon the card are the following:

Gravitational and accelerative forces upon the symmetrical mass of the card.

Gravitational and accelerative forces upon the counter-weight.

Gravitational and accelerative components of the buoyant forces on the card.

Viscous forces between card and liquid.

Magnetic forces on the card.

The resultant motion has been most carefully analyzed by Lindeman, and while the general equations of motion cannot be integrated his discussion of the initial motions leads to conclusions verified by experiment and indicate how to minimize certain unavoidable faults.

His first conclusion is that the card will always set its plane very nearly normal to the "apparent gravity" on a bank. This being true, simple considerations suffice to prove the existence, though not the magnitude, of the most troublesome characteristic of the compass, the so-called "northerly turning error." If one considers a plane taking a bank to turn from a north course toward the west, it is readily seen that the banked card will have a magnetic couple acting on it, tending to turn the card in the same sense in which the case which carries the reference or "lubber" line is turning. The same thing will hold for a turn from north toward east, and it is found that under some conditions the card will turn farther than the case, thus indicating a turn in the *wrong direction*. For a plane turning out of a south course, the card will turn in the opposite direction to the case and hence will exaggerate the amount, though this cannot lead to uncertainty as to the sense of the turn. Ambiguity only occurs for a turn out of the north. Lindeman's final conclusion is that under ordinary conditions as to speed a card having a period of from 40 to 60 secs. will not indicate a wrong turn, whereas with a period of 20 secs. or less the wrong turn will be shown.

The first experimental study of the behavior of airplane compasses, with approximate discussion of the cause, was carried out

by the late Keith Lucas, who pointed out that, because of the lack of symmetry of the card, there is an accelerative force tending to make the south (counterweighted) end of the card point *away* from the axis of a turn. This conspires with the magnetic couple referred to above, and becomes very pronounced in a spin. If this is eliminated by using a symmetrical card with double pivot, the magnetic effect is sufficient to produce an ambiguity. The errors we have been discussing are only troublesome in case the compass is the only means of maintaining a straight course, in

FIG. 8.

fog, or cloud flying, and then they will be exceedingly dangerous. While there is no doubt of the advantage of a long-period compass (R.A.E. Mark II, for example) as regards northerly turning error, it has the disadvantage of a slower recovery, and for this reason has not been as extensively used as it otherwise would have been. Fig. 8 shows the most recent English compass, the aperiodic, evidently designed to combine the advantages of the two types, in which the mass of the card is reduced to a minimum and the damping relatively increased. I have seen no reports of its behavior.

Some early English reports laid great stress upon the desira-

bility of having a perfectly spherical bowl in order to reduce the disturbing effects of currents in the liquid produced by rotation, but our experiments, and I think the later English ones also, showed that this was of relatively little importance.

There are still to be considered two most troublesome matters, the disturbance due to vibration and to magnetic fields on the plane. Vibration may not only produce a very annoying motion of the card, interfering seriously with the accuracy of reading, but also a steady deflection of from 2 to 15 or more degrees, depending upon the nature and amplitude of the vibration, the method in which the compass bowl is supported, the shape of the pivot and cup, the form of the guard and the net weight on the pivot. The bowl must be supported so as to maintain its alignment and at the same time to permit a damped motion with respect to the ship. An elastic support without damping will not do. A compass which will work upon one plane may not work upon another, as we found to our cost. It is not necessary to go into these details but they must be studied carefully for each type of installation.

The magnetic troubles must also be studied for each type of plane and position of the compass, as they depend on the location of iron parts, and upon the type and position of the ignition mechanism. Compensation of the magnetic field due to the plane is very difficult, as they are likely to vary from time to time, and are partly due to induced magnetization which varies with the heading of the plane. This is particularly true of the pilot's cockpit next the engine of a high-powered plane having a battery-ignition system. It is desirable to check the results of "swinging ship" on the ground by tests in the air to determine compensation and vibration errors, and this requires special methods. Perhaps the simplest is by the use of a stabilized horizontal graduated circle with a vertical wire at the centre, the position of whose shadow on the circle can be read off. This gives the apparent bearing of the sun with respect to the axis of the plane and from this, knowing the day and hour, the true bearing may be computed and compared with the compass bearings taken at the same time. Mr. Sterling demonstrated the accuracy of this method in many of our tests.

Turn Indicator.—While this instrument has not yet come into general use, it is sure to be of the greatest importance, for it re-

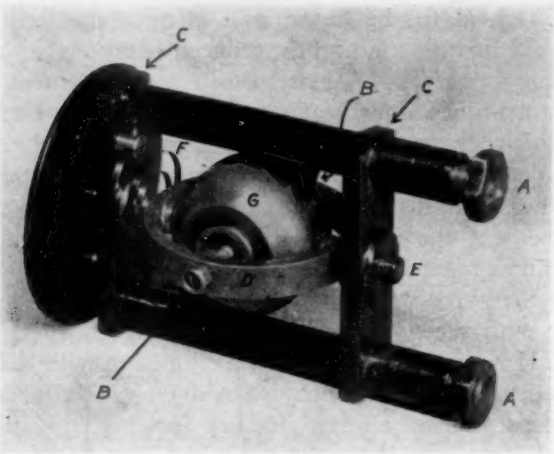
lieves the compass of the second and most difficult demand upon it, assistance in holding a straight course. Two types have been developed: the first, depending upon the difference in air speed of the two wing tips during a turn; the second, using gyroscopic precession. In the first case two pitot tubes may be mounted, one at the tip of either wing, connected to a differential pressure measuring instrument in the cockpit. It works out that the difference in impact pressure between the outer fast-moving wing and inner slow-moving wing tips is just twice the outwardly directed centrifugal pressure difference in the connecting tubes due to the rotation, hence the net pressure difference available for measurement is equal to the latter, and is quite small in amount. On a banked turn this is slightly reduced because the outer wing tip is at a higher level. A more sensitive arrangement would be to use venturi tubes in place of pitots, and we experimented quite a little with such an arrangement at Langley Field. In either case it is desirable to pivot the tubes and provide a tail vane so that they head into the air stream. At best the venturi tubes, and to a less extent the pitot, gave irregular and fluctuating indications, and as regards sensitiveness, convenience, and small head resistance the gyroscopic type is much superior.

Work with the gyroscopic type in this country was started by Dr. A. H. Compton, working in conjunction with the Air Service, on a device originally intended as a gyroscopic inclinometer,[2] but which proved to be unsatisfactory for this purpose and was redesigned for a turn indicator. We had been anticipated, however, by Prof. J. B. Henderson in England. The essential feature is a gyroscope mounted with its axis parallel to the floor of the ship, and arranged so that if, due to the turning of the airplane, one attempts to turn the gyro axis around a vertical axis, it will precess around a horizontal axis and thus indicate the sign as well as the rate of turning. To control the motion of precession, springs are provided which tend to keep the gyro frame symmetrical with respect to the case, and also some form of damping. Several forms of drive have been tried for the gyro, an airscrew

[2] The Germans also developed a gyroscopic turn indicator, combined with an inclinometer, which appears to be based on the same idea which occurred to Doctor Compton and which did not prove satisfactory as an inclinometer because so sensitive to lateral accelerations. A German instrument of this sort is now in the possession of the Bureau of Standards.

with flexible shaft, air jets, and Doctor Henderson's form in which the rotor is itself the airscrew. With proper design extreme sensitiveness may be obtained, so that small angles of bank may be considerably exaggerated, and of course the instrument will indicate unbanked turns as well. The design which we worked out, shown in Fig. 9, is perhaps the lightest and simplest, and most flexible, since the indicator may be placed at any convenient place, the small suction venturi being preferably in the slip stream. Doctor Henderson's is more rugged and has the advan-

FIG. 9.



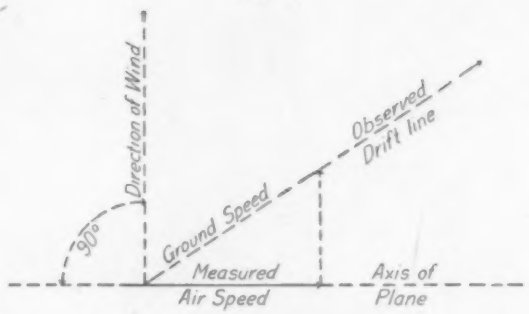
Turn indicator: The frame is hollow and carries air through the openings *AA* to the jets *BB* which drive the gyro *G*.

tage of being in a single unit. Furthermore, by the use of some form of integrating device he proposes to show the total change in course which has occurred since the last setting of the instrument. The value of a turn indicator in cloud or night flying is obvious, as it enables the pilot to fly straight, avoiding the turns and spirals which throw a compass out of commission. A turn indicator and a properly installed compass form a strong combination.

Turning now to the general problem of aerial navigation, it is obvious that it differs from that of ordinary navigation in two important respects: first, the addition of the third dimension, and second, the high velocity of the currents (winds) in which the

vessel is moving. The method of handling the third dimension, altitude, has already been discussed, and the significance of the second factor must now be considered. Navigation in general may be carried on in two ways—either by the independent determination, by astronomical or wireless observations or otherwise, of successive positions, or by attempting to determine the *change* of position from a known starting point, *i.e.*, "dead reckoning." The latter involves determining the direction of a course with respect to the ground and the speed over the course. In sea navigation the velocity of the currents (which correspond to the winds in the case of the airplane) is usually small compared to the ship's speed, and hence easy to allow for. On the other hand, in the case of air navigation the wind velocity may be as high as

FIG. 10.



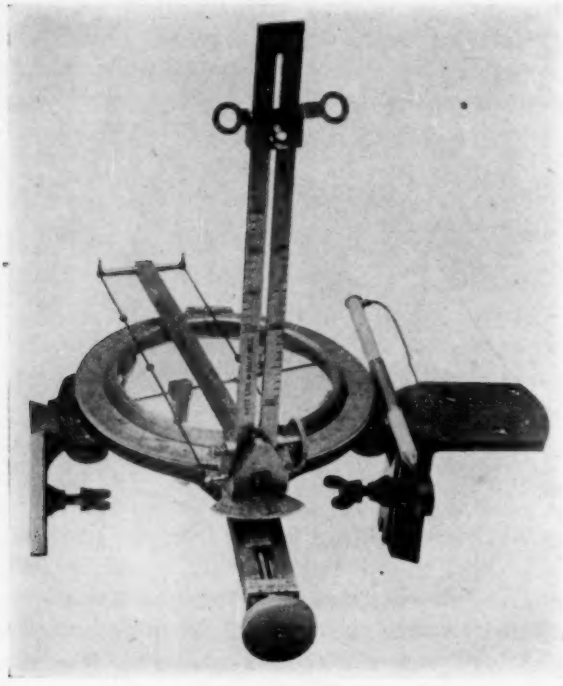
that of the plane, so that the speed with reference to the ground (ground speed) may be quite different from the air speed, and the direction of the "course made good" (with respect to the ground) quite different from the "course steered" (axis of plane, assuming no side slipping). It is therefore *essential* for "dead reckoning" that the ground or the sea should occasionally be visible in order that the ground speed may be measured and the ground course, called line of drift, be determined by using some form of drift meter.

Drift Meter.—This may involve a wire, or series of lines on a glass plate, which can be rotated with respect to the plane axis until parallel to the apparent motion of objects on the ground, the angle of the line of drift with respect to the axis of the plane being then read. Or a series of radiating wires may be used whose angle with respect to the axis is known, as in Captain

Webster's device, so that one can note the line along which objects drift. With the latter form the drift angle may be determined to 1°. At the same time the compass reading must be noted, and from these two the bearing of the drift line, or course made good, is at once obtained.

The ground speed may be determined in several ways, one of which is as follows: First, head the plane so that the drift is

FIG. 11.

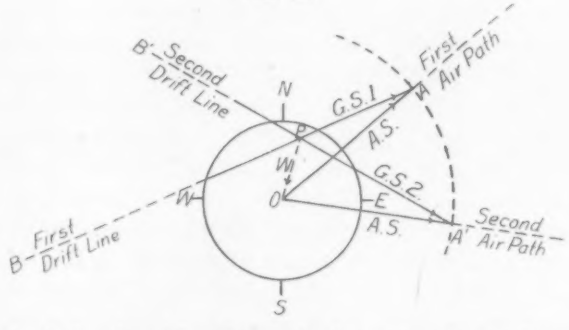


parallel to the axis, *i.e.*, fly up or down wind. Then, using the compass to determine the angle, turn and fly at right angles to the wind and determine the drift angle. We then have a triangle (Fig. 10) for the mechanical solution of which several very ingenious devices have been designed, of which those of Major Wimperis may be particularly mentioned. The procedure of flying along and at right angles to the wind would in general involve considerable departure from any given course, which it is

desirable to avoid. The Wind-gauge Bearing Plate (Fig. 11) of Major Wimperis enables one to determine the ground speed by either of two methods without departing more than 25° from a given course. The geometry is as follows (Fig. 12):

If AB and $A'B'$ are two traces of the drift on a transparent-bearing plate which is kept properly oriented with respect to the compass points, and if AO and $A'O$ are in the corresponding directions of air speed, and proportional to these speeds, then we know that the wind vector must have one end at O and the other end on both AB and $A'B'$, and hence at their intersection P. PO is therefore in direction and magnitude (on the scale of AO) the wind velocity, and AP and $A'P'$ the two ground speeds. The

FIG. 12.



"wind point" having been located, the ground speed may be at once determined without observation for any course and air speed, so long, of course, as the wind remains constant.

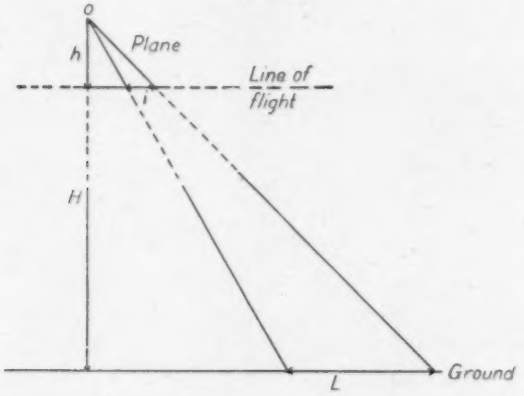
The ground speed may also be determined by timed observations of drift between fixed points on a drift bar, swung so as to be parallel to the line of drift. If h (Fig. 13) is adjusted for altitude so that $h = kH$ then $l = kL$, and k and l may be so chosen that $L = 1$ mile, in which case the ground speed will be 3600 divided by the number of seconds taken for an object to pass along l. In many types of instruments scales are provided which automatically carry out this and similar computations.

Various other schemes have been proposed and tried as ground-speed meters, with the general object of reducing the calculation involved and increasing the accuracy. Some of the most ingenious, which have not yet been made practicable, involve a moving index or tape whose speed can be adjusted until it syn-

chronizes with the apparent ground speed; the device could be graduated to read directly the ground speed since if $h = kH$, then $s = kS$, where s and S are, respectively, the index speed and ground speed. It should be remarked that for ground speed observations over the sea smoke bombs may be dropped to serve as reference points by day, and flares by night.

Navigation by the determination of position, the only method available when flying over clouds, involves either astronomical or radio observations. For the former the primary instrument is the *sextant*, which, for cross-oceanic flights, is likely to be of very considerable importance. Its use was the subject of careful

FIG. 13.



experimentation during the war by Doctor Russell, Captain Webster and Captain Gault at Langley Field. The fact that the horizon is frequently invisible from the upper air, being obscured by haze if not by clouds, makes it necessary to use either some form of stabilized mirror (artificial horizon) or a level or bubble sextant. At Langley Field a damped pendulum stabilizer, and a form of bubble sextant devised by Dr. R. W. Willson, were used, and both are subject to errors due to lateral acceleration. With careful flying the average error of a single observation amounts to only 13′ of arc, but with ordinary piloting it is 26′, while the average error of six observations is only 6′, with careful piloting. While the accuracy of the two instruments was about the same, the bubble sextant proved to be the most convenient. When two heavenly bodies are visible, a location with an average error of

seven miles may be made in ten minutes, using the natural horizon, and in twenty minutes with an error of fifteen miles, using the bubble sextant. The British Air Service has developed a different type of bubble sextant, designed so as to permit the observer to look *directly* at the object observed instead of at the horizon, with the idea that in the case of stars at least, such an arrangement will make it easier to find the proper object and point on it.

Directional wireless is of great promise as an aid to aerial navigation, as well as to navigation of the sea, though the very large anomalies and uncertainties, amounting at times to 90° or more, which have been discovered and studied during the past two years at land stations, make it appear very doubtful whether long wave transmission will ever be useful for this purpose. Short and discontinuous waves seem to give indications of direction reliable to perhaps 2°, but the use of short waves will very much limit the range. There is still the possibility, though not the probability, that the anomalies will be found less in magnitude in the air and over the sea. Directional wireless may be used in several ways. For example, the plane may send out a signal, which may be received by several Direction Finding stations. The several bearings of the plane being collected at one station and plotted, the plane's position may be determined and sent back to it by radio. Again, the plane may carry a direction-finding set, the bulkiest part of which is the coil three or four feet square, with which the bearings of certain known stations may be taken, and from these the position of the plane may be quickly found graphically. In the case of smaller planes, it is necessary to fix the direction-finding coil with respect to the plane and turn the plane to face successively the various sending stations. In either case the compass, or a special neutral gyrostat with horizontal axis, must be used to give a fixed direction from which bearings may be taken, but either a compass or a gyro will maintain a fixed direction more accurately in a plane which is itself not turning, so that this as well as the greater ease and speed of observing is an argument in favor of the rotating coil whenever its use is possible.

Stabilizing Devices.—The primary purpose of all such devices is to indicate, or to adjust something to, the vertical. Hence the arrangement must be sensitive to gravity—hence no "neutral" gyrostat or "neutral" inertia body will do. A pendulum is the simplest form of vertical indicator—and for some purposes is very

useful, especially if provided with internal instead of external damping, as has been done in various forms of gyroscopic pendulums. Doctor Duff's gyro damped pendulum bombsight is an example. An absolutely neutral gyro mounted in gimbals would of course be useless for our purpose, since even if it were initially given the correct vertical position, its tendency to maintain a fixed direction in space would soon throw it off because of the earth's rotation, or the motion of the aeroplane on the earth, or both. We must therefore have a gyroscope mounted so that there is a gravitational or other couple tending to keep the axis vertical. The less this couple, that is, the more nearly neutral the gyro, the more slowly will it respond to lateral accelerations—but also the more slowly will it return to vertical after being disturbed. But even with such a couple, if there were no friction in the gimbal bearings the gyro would not return if disturbed, but would *precess* around the vertical, a real gyroscope being gradually brought into the vertical by friction of the gimbal bearings. Hence various devices called " erectors " have been used to introduce a couple at *right angles* to the plane containing the axis and the vertical, which will restore it to the vertical when disturbed.

All of these involve in effect one or more free short-period pendula, and it is to the average vertical as defined by these that the gyro will tend to be set. As such a pendulum will quickly adjust itself to the " apparent " vertical it will, unless otherwise provided for, at once bring to bear a couple tending to move the gyro into line with the apparent vertical, the " erector " becoming a " disturber." In ordinary use it would be possible to keep the gyro axis at all times within, say, 8° of the *true* vertical, so that it would not be necessary for the erector to act beyond this range. Hence there would be an advantage in arranging to have the erector automatically cease to operate when the " apparent " vertical departed by more than, say, 8° from the true vertical. This may be very simply accomplished with some types of erectors. It would also be convenient to have means whereby either the gyro could be clamped, or the erecting mechanism thrown out of action when the disturbing forces were very large.

Among the erecting mechanisms which have been devised mention should first be made of that of Dr. J. G. Gray, which has achieved very considerable success. The essential feature is a revolving disc having specially curved slots in which roll a num-

ber of steel balls, the whole being rigidly attached to the gyro frame, and the disc being slowly rotated by either an electric motor or air turbine. The plane of the disc is ordinarily normal to the direction of gravity, in which condition the balls are symmetrically arranged around the axis of rotation. If the gyro and the disc are displaced out of this symmetrical position the balls roll, and, due to the combined action of the rotation of the disc and the curvature of the slots, the displacement of the balls is such as to give a couple in a plane at right angles to the plane in which the gyro axis has been moved, which tends to erect the gyro. Of course the erector also tends to move the gyro axis out of the true vertical into the apparent vertical. The magnitude of this couple and hence the rapidity with which the disturbed gyro is restored to the vertical may be increased by increasing the speed of the disc. This very ingenious device has been further developed than any other, and shown to be practicable and reliable, but there does not seem to be any easy way to limit the range through which the erector acts.

A second scheme has been worked out by the Sperry Company and Doctor Lucian of the Air Service. This involves two short-period pendula, swinging around the gimbal axes of the gyro in the fore and aft and transverse planes, each carrying a roller contact which plays over a two-part metal contact arc. In the symmetrical position, when apparent gravity is parallel to the gyro axis, neither roller makes contact with either half of its arc, but if the gyro is displaced in either plane a contact is made which actuates an electro-magnet in such a way as to introduce a torque at right angles to the plane of displacement, which tends to erect the gyro.

A third form, which was extended and improved by the Air Service, originated, I think, with one of the early forms of the gyroscopic compass. This again involves two short-period pendula each of which, in the symmetrical position, covers two small air outlets. When displaced, one or the other of these is opened and the reaction of the escaping air-jet is in the plane perpendicular to the displacement and in such a direction as to erect the gyro.

Such gyroscopic devices as we have been describing are of fundamental importance as auxiliaries attached to bomb-sights, cameras, artificial horizons, ground-speed indicators and other

apparatus which it is desired to maintain as nearly as possible in a fixed relation to the true vertical. Attached to a double pivot symmetrical compass, they would eliminate the turning errors of that instrument, but at the cost of undue complication; attached to a bomb-sight they would eliminate the largest errors of bomb-dropping, as was about to be practically demonstrated on a large scale just at the close of the war. They also contribute an essential element to the successful application of aerial photography to accurate topographic mapping. It is difficult to state the accuracy with which they will operate, as this depends entirely upon the conditions of use, but it is safe to say that in a carefully piloted plane the best of them would certainly maintain the true vertical to 1° or less.

There remains to be considered only one class of instrument, all of which are still in the stage of a dream rather than a realization, that is, absolute position indicators which would indicate at any time, without any outside observations, the absolute position (latitude and longitude) of a plane. So far as known, experiments and suggestions have been along the following lines:

1. The use of the direction of total magnetic force.

2. The gyroscopic maintenance of a fixed direction, compensated for the earth's rotation.

3. The double integration of acceleration to give velocity and then distance travelled (really automatic dead reckoning).

All of these would involve, among other things, a satisfactory stabilizing device, but the additional difficulties are so great that they have so far proved insurmountable. I feel that this problem of a position indicator is a sufficiently difficult one to leave to the readers of the JOURNAL, and worthy their best efforts.

The High Frequency Spectra of Lead Isotopes. C. D. COOKSEY and D. COOKSEY.—Aronberg, corroborated by Merton, has found a difference of .0043 Angstrom between the wave-lengths of a certain line of ordinary lead and of lead of radioactive origin. Others have investigated the X-ray spectra of lead and have stated that the spectra of ordinary and of isotopic lead differ in wave-length by only a few ten-thousandths of an Angstrom. The authors confine their work to a single line of the L-series and conclude that the two varieties of lead give lines which differ in wave-length by less than .005 per cent.

G. F. S.

INITIATION OF MILITARY EXPLOSIVES.*

BY

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I. INTRODUCTION.

THE first use of an initiator—in other words, of a small amount of some sensitive substance through which explosive action of a less sensitive main charge might be brought about—came in the early part of the nineteenth century. The slow and cumbersome matchlock had given way to the flintlock during the previous century, but, even with this improvement, the time between the decision to fire and actual ejection of the bullet was still considerable, loading was slow and difficult, and the action was by no means certain. In 1805, the Rev. A. J. Forsythe, a Scottish clergyman, proposed to the British military authorities the use of a mixture of potassium chlorate, charcoal, and sulphur as a primer for small arms; a number of experiments were made, but the method was not adopted. Nine years later, several men, of whom J. Shaw of Philadelphia seems to have been the first, independently developed the percussion cap loaded with fulminate of mercury. With this cap, our modern methods of igniting propelling charges came into being; the only essential changes since then have been the introduction of electric firing for certain purposes, and the interposition of auxiliary ignition between the initiator and the main charge to ensure proper initiation of large charges.

The invention of the percussion cap brought about another development, and one which, perhaps, the inventors themselves did not foresee. As early as 1588, an Italian refugee in the employ of the Dutch, had used an explosive shell filled with black powder, employing as a fuse a tube or pipe filled with slow-burning powder which was ignited by the discharge of the gun. Such shells were only of use against material, for men usually had plenty of time to get out of the way before they exploded. Efforts were made to improve timing and to get

* Communicated by Dr. Allerton S. Cushman.

greater certainty, but this type of fuse was never satisfactory even in the low velocity artillery of that period. The application of the percussion cap to this problem was soon seen; in 1846, Quartermaster Freeburn of the British Army brought out a time fuse in which a percussion member was operated by the shock of discharge of the gun, and four years later Commander Moorson, of the British Navy, invented a fuse firing on impact. In 1853, the Russian fleet made the first use of the percussion fuse, against the Turkish at Sinope; the effect was great.

This was only the first step in the development of the modern shell—two further discoveries were necessary to its development. The first of these came in 1864. In 1859, Alfred Nobel, a Swedish engineer, became convinced that nitroglycerine, which had been discovered eight years earlier, was destined to be of great importance to the engineering profession. It was admittedly the most powerful explosive known at the time—perhaps the most powerful that ever would be known. Unfortunately, it could not then be used, for the only practical way of setting off an explosive for use in blasting was by a spark or flame, and nitroglycerine, instead of exploding under such conditions, would usually only burn quietly. After five years of constant research, Nobel found that a small charge of fulminate of mercury, which can be exploded readily by heat or flame, would completely and surely detonate a large charge of nitroglycerine. "The step from the use of fulminate of mercury in caps for the ignition of powder in weapons to its use as a means of converting the igniting flame of safety fuse into the percussive effect necessary for the ignition of the new explosives may at the present time seem to be a small and simple one, but nevertheless, as has been rightfully acknowledged, it is the one which has made possible the commercial use of nitroglycerine and most modern explosives." (Colver, p. 5.)

Nobel's invention was immediately applicable to mines and torpedoes for military use, and in 1868, E. O. Brown, a chemist attached to the British Ordnance Department, demonstrated that fulminate would detonate gun-cotton, which had been discovered in the same year as nitroglycerine, and which was being investigated by the ordnance departments of several nations. It was quite different with shells; here the filler must be sufficiently insensitive to shock to withstand discharge from the gun with-

out exploding. Experiments made with the powerful new explosives had failed on this account; some chemists of the day, indeed, feeling that sensitiveness and power were closely related in the organic explosives, held that an explosive sufficiently inert for use in shells would show no particular advantage over black powder. The possibilities of the trinitrated benzene derivatives had not yet been recognized, although several of them had been prepared, and one, trinitrophenol or picric acid, had been known for a hundred years and had been used extensively as a dye. It was not only that these substances are difficult to explode by any ordinary means; in addition to this, it was then considered that, for good effect, the oxygen content of an explosive system must be sufficient to burn all the carbon present to carbon dioxide, and that an excess of oxygen was desirable. By this reasoning, it appeared that compounds of the picric acid type, which do not contain enough oxygen to burn the carbon present even to the monoxide, would only be of value as a substitute for charcoal in the familiar black powder mixture.

The first hint of the possible use of any of these substances was given by Sprengel, a noted explosives chemist, in a lecture delivered before the chemical society in London in 1873. In the course of this he stated: "It is here noted that picric acid alone contains a sufficient quantity of available oxygen without the addition of any foreign oxygen carrier for it to be an extremely powerful explosive, provided that it is ignited by means of a detonator." The full significance of this statement was not realized, and no further developments were made until 1885, when Eugene Turpin, a Frenchman, announced that, provided the initial impulse was sufficiently violent, pure picric acid was one of the most powerful of explosives, and announced to the world its value as a filler for shell. The design of shell he proposed, in fact, is essentially the one used to-day. After more or less hesitation, for it seemed inconceivable to many authorities that an explosive of such power could be at once reliable in action and safe in the bore of the gun, picric acid was universally adopted as a shell filler (lyddite, melinite, schimose, pertite, etc., are all either pure picric acid or mixtures consisting mainly of this substance). The subsequent replacement of picric acid by other explosives, notably trinitrotoluene, detracts in no way from the importance of Turpin's work; the principal, and often the only

components of these explosives are nitrated benzene derivatives like picric acid, and therefore of a class generally neglected before Turpin's discovery.

It will be noted that a differentiation has been made between the initiation of propellants and of high explosives. The two actions are quite different. Propellants are explosives of the first order, which may be exploded by direct action of a flame; the high explosives used are of the second order, and full explosive effect is to be obtained only when they are subjected to an intense initial impulse, such as that given by the detonation of another explosive. Propagation of the explosive impulse through propellants is slow, being never much in excess of one metre a second; the velocity of propagation through a high explosive may exceed 8000 metres per second. For best results with propellants, the initiating effect should be constant—an increase as well as a decrease of the effect is undesirable; the value of high explosives is dependent upon their energy being released in a very brief time, and their effectiveness increases as the time required for the explosive reaction is decreased; it follows that an increase of initial impulse above the standard is never detrimental, and may be advantageous. The initiation of propellants, or ignition, and that of high explosives, or detonation, will therefore, be considered separately.

II. IGNITION AND THE IGNITION SYSTEM.

The requirements of the ignition system are as follows:

(*a*) Certainty of functioning; a single firing impulse must be sufficient to cause ignition;

(*b*) Rapidity of action; many weapons, such as small arms and naval guns fired by gun pointers, can be held on the target only for a brief interval of time;

(*c*) Regularity of ballistics; the charge must be ignited in such a manner as to give the desired muzzle velocity to the projectile, without causing pressures in the gun in excess of those set by the gun designer;

(*d*) Regularity of firing interval; this quality becomes of importance with naval guns fired by director, and aeroplane machine guns firing through the propeller.

Under the conditions actually obtaining, these requirements

will be satisfactorily fulfilled if the action of the ignition system is sufficiently uniform. Smokeless powders differ enough from lot to lot to make it necessary to determine the weight of charge for each lot experimentally; this procedure automatically takes up any constant ballistic error due to ignition. Assuming that the system is so designed as to give the required rapidity of action—and the experimental data indicates that this is generally true—the only requirement in practice is that the igniting effect always be the same.

There is little definite quantitative data available upon the effect of changes in ignition. As in all problems connected with interior ballistics, it is difficult to control experimental conditions to a satisfactory degree, to obtain accurate data, and to analyze correctly the results obtained. It is certain, however, that changes in ignition effect exert their influence by changing the rate of combustion of the charge. The rate of combustion of propellants is a direct function of the temperature and pressure, and the changed initial conditions resulting from a change in ignition tend to persist throughout the decomposition; one of the advantages claimed for powders of the cordite type, which contain nitroglycerine as well as nitrocellulose, is that their rate of combustion is less sensitive to changes of pressure than that of the "straight" nitrocellulose colloids, and that, therefore, ballistic results are more uniform.

Ignition less than the standard is far more common than excessive ignition. Its effect is to decrease the rate of combustion of the charge. As a result, the maximum pressure is lower and occurs later, the muzzle pressure is higher, and the muzzle velocity somewhat lower, since more of the energy of the propellant is lost through the muzzle after ejection. Dispersion will be increased somewhat, partly because under conditions of slow combustion the powder itself does not burn with the best regularity, partly because high muzzle pressures of themselves tend to cause erratic flight of shell. With still smaller ignition effect, and a still slower rate of combustion, the projectile will leave the muzzle before all the powder has been burned; a decided drop in muzzle velocity and a considerable increase in dispersion will result. At the same time, excessively high pressures may occur, due to "waves" of pressure (this phenomenon is not uncommon with powders too slow for the gun). Complete

failure to fire the main charge may be regarded as the limiting case of insufficient ignition. In addition to its effect upon ballistics, insufficient ignition will at the same time cause increase and irregularity of the firing interval, thus bringing about still further loss of accuracy under service conditions.

The tendency of excessive ignition is to increase maximum pressure, to cause slightly greater muzzle velocity, and to reduce the firing interval. Of these variations from standard, only the first can ever be great enough to be of importance; practically, it is almost impossible for the excess in ignition effect in service to be enough to cause dangerous pressures. Excessive pressures in service, if due to ignition, are apparently quite as likely to be caused by insufficient ignition effect.

Part of the trouble in the ignition system is due to the powder itself. Smokeless powders, and particularly the "straight" nitrocellulose powders, are rather hard to ignite, and burn very slowly at atmospheric pressure. In all except the smallest guns, it is necessary to employ an ignition train of more inflammable explosives, not only to ensure adequate ignition of the propellant, but to build up the pressure somewhat so that the initial rate of combustion may be higher. The action is simple—the initiator, in exploding, ignites the inflammable ignition charge, and this in turn sets off the main charge. Not uncommonly there is an additional step—the first ignition charge, instead of acting directly upon the main charge, sets off a second and larger ignition charge, and this fires the propellant itself; other ignition charges may be distributed through the main charge. The last stage in the ignition train usually consists of black powder; the intermediate stage, where used, may be of finely-divided black powder, or of an even more sensitive and quick-acting substance. The first ignition charge is assembled in the same unit as the initiator, thus forming the "primer"; the final charge in the train, where more than one is used, is usually assembled with the charge. The size and distribution of the ignition train, especially of the final stage, varies considerably with different designers.

The ignition train tends to make for regularity of ignition effect even when the action of the initiator—the member most likely to function badly—is not satisfactory. The substances

used in the intermediate stages have a sufficiently high rate of combustion to give about the same effect even when not ignited thoroughly. Poor functioning in other than the final stage, however, tends to lengthen the firing interval somewhat, and does cause a decrease of ignition effect; for instance, a change in the distance between two members of the train may cause a distinct change in ballistics. A defective final stage—such as may be produced by allowing the black powder to get wet—will of course always result in insufficient ignition. It has never been conclusively proved that variations in size of standard ignition charges (provided, of course, they are not made too small) or in distribution, make any considerable difference in regularity; the constant change in ballistics which results is, of course, taken up in fixing the charge at the proving ground.

In small arms, the initiator is required to ignite the propellant directly; with this system, ignition irregularities are far more common than in larger guns. It is commonly believed, in fact, that the primer (or initiator) has more effect upon the ballistics of small arms than any other factor. In the first place, the margin of operation is much smaller—an initiator which can set off the inflammable explosives used in the ignition train may entirely fail to ignite smokeless powder. Again, the substances used for initiators, as will be seen later, are from their very nature especially liable to deterioration in storage, with consequent change in ignition effect. Not infrequently the charge does not fill the cartridge case completely and the distribution of the charge around the primer differs from round to round, with a corresponding change in the ignition effect of the initiator; this is the foundation for the belief that a rifle or pistol shoots more accurately if pointed vertically first and then brought down on the target. Finally, the tendency toward regularity of action found in the ignition train is absent here; variations in action of the primer will inevitably result in corresponding variations in ignition effect. The space available is too small to permit use of a train; the problem, therefore, resolves itself into a question of obtaining an initiator which will have an adequate margin of effective operation, will be constant in its action, and will retain these qualities under the various conditions encountered in storage and in service.

III. DETONATION.

In order to obtain full explosive effect from explosives of the second order—the class which includes all military high explosives—it is necessary to use a small charge of an explosive of the first order. The high explosives in use, as has already been pointed out, can be employed in shell only because they are insensitive to shock or impact; when ignited, they usually burn, if at all, rather slowly and quietly, and detonation, if produced, is rarely complete. To insure rapid and complete release of the potential energy of these substances, a strong initial impulse such as that obtained from another explosive is required. There is, in fact, a close connection between initial impulse and effectiveness. "The character and intensity of the initial impulse influence the course of an explosive reaction to such a large extent that it has been said, and not without justification, that the effectiveness of an explosion depends more upon the initial impulse than upon the explosive itself." (Brunswig, p. 31.)

The manner in which the explosion of one substance brings about the explosive decomposition of another has been the subject of experiment and discussion ever since Nobel announced his discovery that fulminate of mercury was an efficient detonating agent for nitroglycerine. The explosive impulse is not transmitted from one charge to the other by the direct transfer of heat, nor does it seem to depend upon the propulsion of particles of the primary explosive against the secondary charge; detonation still occurs when a film of water, glass, or metal is interposed between the two charges, although a distinct loss of explosive impulse results from such interpositions; it has been found, indeed, that the substitution of a film of water for an air-gap has resulted in an increase of initiating effect.

Abel brought forward, in 1869, the view that the action was due to synchronism of molecular vibrations; he had found that 6.5 grams of nitrogen iodide is insufficient to detonate nitroglycerine or gun-cotton, and that 3.25 grams of nitrogen chloride was required, while 0.3 grams of fulminate of mercury, which is less powerful and has a lower velocity of detonation, was always enough to produce satisfactory action. This hypothesis has never been accepted. According to it, each explosive should show such perfect synchronism with itself that it would furnish the best detonator for itself (Victor Meyer). Gun-cotton ex-

ploded over nitroglycerine will cause detonation of the latter, but the converse holds only when the charge of nitroglycerine is large; if wave synchronization is assumed, the action must be completely reciprocal (Berthelot). If the various explosives be arranged in the order of sensitiveness, as determined by the minimum quantity of a given detonator agent required to detonate completely a fixed weight, the order will be different with different detonating agents (Brunswig). Some authorities feel, however, that the wave-synchronism theory should not be totally disregarded; for instance, the detonation of a cast or pressed charge of the trinitrated benzene derivatives, such as picric acid, is accomplished best through a "booster" charge of material of the same class as the charge. (Colver, p. 595.) The efficiency of the nitro-aromatic explosives as boosters, as compared to nitroglycerine, for instance, has not yet been explained in a satisfactory manner.

The explosive-wave theory of Berthelot, is the one generally accepted to-day. According to his idea, the transmission of the explosive impulse differs in no way from the propagation of the impulse through a charge; transmission and propagation both take place through the agency of an explosive wave, which also has the power of propagating itself, and therefore of bringing about explosion, through intervening mediums. The wave is considered as a recurring cycle of released and transformed energy, having four phases:

1. *Transformation of Mechanical Energy to Calorific Energy.*—A part of the mechanical energy set free in one layer of the explosive is converted instantly into heat energy in the adjacent layer by reason of the impact of molecules. Since the movement of the molecules is radial, the efficiency of this conversion is dependent upon the shape of the charge and the location of the detonator within it; for a long thin column of explosive detonated at one end, the efficiency will be less than 50 per cent., since the molecules are confined by the explosive only on one side; if, on the contrary, the action is taking place in the interior of a mass of explosive, the efficiency will be practically 100 per cent. The mechanical energy that is not converted into heat energy exerts pressure on the confining medium and thus becomes the vehicle through which work is accomplished. The thickness of the layer of the explosive which enters into this

phase is not only dependent upon the amount of energy available, but varies with the physical properties of the explosive material, principally with its elasticity. The less elastic the explosive material, the thinner the layer, and hence the more times the cycle will recur in a unit length of the explosive material. This phase, to put it briefly, subjects the layer to sudden heating under great pressure.

2. *Transformation of Calorific Energy to Chemical Energy.*—Some of the calorific energy of the layer is used to overcome the chemical stability of the explosive material, and thus to release the potential energy of the layer; the rest is used to accelerate and reinforce the chemical action.

3. *Transformation of Chemical Energy to Calorific Energy.*—Once the chemical stability of the material is overcome, its potential energy is converted into kinetic energy—largely calorific energy; all military explosives develop heat on detonation. This phase differs from the others in that it supplies kinetic energy to the cycle, while each of the others consists only of a transformation of the kinetic energy obtained from the preceding phase, and therefore can have no more kinetic energy than the preceding phase is capable of transferring.

4. *Transformation of Calorific Energy to Mechanical Energy.*—The large volume of gases formed from the layer of explosives is in an extremely active state of molecular vibration, and these molecules are therefore manifesting their energy as kinetic energy. (Bulletin 59, p. 9.)

The full action of a simple explosive system, according to this theory, is essentially as follows: sufficient heat energy to overcome the chemical stability of part of the detonator is supplied from outside the system, either by direct action of a flame or incandescent solid, or by the conversion of the kinetic energy of a blow into heat; the explosive wave is set up by the resulting decomposition of part of the detonator and passes rapidly from layer to layer until all the potential energy of the detonator has been set free, mostly in the form of mechanical energy; this mechanical energy the explosive wave transmits to the main charge, where it initiates the decomposition of the first layer and propagates itself through the explosive as already described.

This theory explains the intimate relationship between initial impulse and explosive effect. If a large amount of the second

order explosive is suddenly decomposed by the direct action of the detonator, the amount of explosive decomposed in the second cycle will be large, a large excess of energy will be available for accelerating decomposition in the second phase of the second cycle, and the time necessary for both cycles will be short, thus reducing the loss of energy by radiation and conduction to a negligible quantity. (The action is so rapid in case of detonation that the decomposition proceeds nearly adiabatically.) Since additional energy is furnished to the explosive wave in each of these cycles in excess of the amount lost as a mechanical energy, the velocity of propagation will increase to the value at which dynamic equilibrium obtains—in other words, to a maximum dependent upon a number of conditions such as the chemical and physical characteristics of the charge. Where only a small amount of explosive is decomposed by the detonator, or where the initial decomposition is slow, as by the action of a flame upon a second order substance, the radiation and conduction losses become significant, the first cycle furnishes little or no excess energy to accelerate decomposition in the second, and the decomposition proceeds as a simple combustion; in some explosives and under some conditions the reaction will die out entirely. It may not be going too far to say that explosives of the second order lack the ability to accelerate burning to the detonation stage, and the detonator supplies the required acceleration. (Tech. Paper No. 162, p. 22.)

The tendency of propagation to proceed at one of two definite velocities is well recognized. "Under conditions usually in force only one or the other limiting value of velocity of propagation obtains in explosive processes, and the corresponding phenomena are accordingly characterized either as deflagration, where the velocity of propagation is only a few metres, or as detonation, where the velocity reaches almost as many kilometres per second." Where escape of energy is prevented, as when the container is very strong, a deflagration can end with a detonation; similarly, where there are hindrances to the propagation of a detonation, such as large gaps in a column of explosive, a detonation may degenerate into a deflagration (Brunswig, p. 80). These phenomena are to be expected in light of Berthelot's theory.

The effectiveness of the explosion, it has already been stated, is largely dependent upon the rapidity with which the energy

is set free—in other words, upon the velocity of propagation. The total energy set free by a low order explosion [1] may be the same as that of a detonation, but the brisance, or shattering effect, is far greater in the latter case; the rate of increase of pressure and the maximum pressure produced are also greater, not only in contact with the charge, but at points removed from it—a matter of considerable importance in constructions in which the radius of effectiveness is of prime importance, such as depth charges. A secondary effect in low-velocity explosions is the driving out of a portion of the charge before the explosive impulse can reach it, with a consequent direct loss of energy. Under certain conditions, to be sure, the finely-divided powder which results will be of some value through its choking effect upon personnel; it is not, however, particularly toxic, and in confined spaces is not even as effective as the carbon monoxide produced by decomposition. (Colver, p. 591; after Lewin and Poggenburg.)

The course the reaction takes, as well as its velocity, may be dependent upon the initial impulse. For instance, of the various ways ammonium nitrate may be decomposed, three are explosive. If subjected to the impulse of a strong detonator, it yields nitrogen, water vapor, and oxygen; a weaker detonator causes decomposition into nitrogen, nitric oxide and water vapor; finally, gradual heating without material increase in pressure results in the formation of nitrous oxide and water vapor, no nitrogen being set free. (Brunswig, p. 32.) The table below (Brunswig, Table 6) gives the heat of explosion, the volume of gas, and the product of these two values—a quantity which Berthelot has proposed as a characteristic expressing the maximum work performed by the explosion.

TABLE I.

Decomposition of Ammonium Nitrate.

Impulse	Heat of Explosion Calories	Gas Volume Liters	Berthelot Characteristic
Strong detonator	421	976	411,000
Weak detonator	140	976	137,000
Gradual heating	151	836	126,000

Even if Berthelot's theory of the mechanism of transmission is accepted, the influence which the chemical and physical charac-

[1] "Low order explosion" is frequently used instead of "deflagration"; similarly, "high order explosion" is synonymous with "detonation."

teristics of the detonator exert upon the effectiveness of the initial impulse still remains to be explained. A small amount of fulminate of mercury, for instance, will effect the detonation of a large charge of an explosive of the second order, although many other explosives apparently more powerful in every way will accomplish the same result, if at all, only when used in large quantities. It would be expected that efficiency as a detonating agent would depend upon the rapidity of explosive decomposition primarily, and upon the energy content to a somewhat less degree, but there is apparently evidence that this does not hold. The velocity of propagation in nitroglycerine has been determined to be over 7000 metres per second, as against less than 4000 metres for fulminate of mercury, and its potential energy is four times that of fulminate, yet it has not the property of initiating detonation in other explosives unless used in large quantities; it has already been pointed out that nitroglycerine is not effective as a detonating agent for guncotton. Abel's demonstration of the detonating efficiency of fulminate of mercury as compared with the highly brisant nitrogen halides has also been referred to. The presence of a heavy metal was for a time thought to be necessary, because the high specific gravity of such compounds allowed high density of loading and correspondingly high maximum pressures, and possibly because the very heavy vapors of the metal had a special value in detonating the secondary charge; organic compounds are now known which are more effective, weight for weight, than any of the metal compounds. (Tech. Paper 162, p. 18.) The particular property or combination of properties which determine the efficiency of a substance as a detonating agent have not yet been successfully determined or measured.

Storm and Cope believe that the initial rate of detonation is the most important property of detonating agents. In view of the difficulty of determining accurately the rate of detonation of detonating compositions, they made a comparison of the relative rates of four such substances by observing the minimum weight of each which would be sure to shatter completely a small open glass test tube in which it was placed, when exploded by rapid heating. The results were as follows:

Mercury fulminate	0.07 gram
Fulminate, 90 per cent.; potassium chlorate, 10 per cent.	0.06 gram
Fulminate, 80 per cent.; potassium chlorate, 20 per cent.	0.05 gram
Lead azide	0.01 gram

Even 0.001 gram of lead azide in every trial blew the bottom out of the test tube. The quantities of the various compositions required correspond with the minimum quantities of these substances necessary to detonate a given weight of an explosive of the second order, indicating a relationship between shattering effect and efficiency as a detonating agent. Storm and Cope believe that, since the particular property of explosives upon which shattering effect depends is apparently the initial rate of detonation, this property must be one of the factors of greatest importance. They further consider it likely that the usual method of measuring rate of detonation, using a column of explosive of considerable length, gives a value of initial rate for these substances much smaller than the true one. (Tech. Paper 125, p. 39.) In other words, the rapidity with which fulminate of mercury decomposes at the beginning of the action is greater instead of less than that of nitroglycerine, for instance, and for this reason it is a better detonating agent than nitroglycerine. The greater efficiency to be obtained by increasing the rapidity of explosion of fulminate of mercury itself has been shown by Wöhler. He found that the finely-divided fulminate obtained by precipitating it with an acid from a potassium cyanide solution at 0° C. was about three times as effective as the coarser original fulminate, as a result of the faster decomposition of the finely-divided material. (Marshall, p. 513.) Taylor and Cope, following a suggestion of Wöhler, reported that the action of fulminate of mercury was considerably strengthened by superimposing a small charge of a quicker-acting explosive. Lead azide and silver acetylide, although the latter is not of itself a good detonator, reduced by one-half the amount of fulminate necessary to fire a given weight of trinitrotoluene.

Accepting the idea of Storm and Cope that the initial rate of detonation is much greater than the value determined by the methods in use, Wöhler's theory may be taken as a basis for further discussion of the matter. He believes that detonation of second-order explosives is initiated by a sudden and enormous local pressure. Any substance, mixture of substances, or combination of substances that, when placed in the detonator shell and ignited, detonates with such velocity that the walls of the shell do not yield until the entire gaseous product has accumulated so that it can escape all at once and deliver by its kinetic

energy a colossal blow, will be an efficient detonator. (Tech. Paper 162, p. 7.) The action is thus dependent primarily upon the rapidity of decomposition, and to a somewhat less degree upon the volume of gas produced (as determined by the composition itself and the density of loading) and to the heat of the explosion.

The views of Taylor and Cope are in general accord with those of Wöhler. In their opinion, the effectiveness of a detonator in practice depends upon explosive energy that may be separated into an intensity factor and a capacity factor. The intensity factor may be called "quickness" and depends upon acceleration of the explosive decomposition. The capacity factor is "strength," and is measured by the total energy of the primer. Of the two factors "quickness" is the more important. According to this idea, greater effectiveness should be obtained by keeping one factor constant and increasing the other, but, if a substance is deficient in one factor, increase of the other should not be particularly effective.. This is actually the case. Storm and Cope found that the amount of sand pulverized by a given weight of fulminate of mercury, compared to that pulverized by the same weight of mixtures of fulminate with 10 per cent. and 20 per cent. of potassium chlorate, is a measure of the efficiency of the respective mixtures. (Tech. Paper 125.) Taylor and Cope established the fact that this sand test is a measure of "strength," and that the effect is, therefore, that of varying the "strength," keeping the "quickness" practically constant. The increased effectiveness of mercury fulminate, brought about by a small quantity of lead azide or silver acetylide, illustrates the case of increasing the "quickness" only. Finally, lead azide could not accelerate silver acetylide (which has the necessary "quickness," but not the "strength") sufficiently to improve its action. (Tech. Paper 162, p. 22.)

Tests of the comparative efficiency of detonating agents, such as those referred to above, are made by determining the minimum weight of the agent which will produce detonation of a given weight of some explosive of the second order, conditions such as density of loading, granulation, and the like being kept constant. The explosive effect obtained from the second order explosive, when detonation is secured, is always practically the same, regardless of the amount of the initial impulse or the agent used. Storm and Cope found, in fact, that the total strength shown by

four nitrocompounds (trinitrotoluene, tetranitromethylaniline or tetryl, tetranitroaniline, and picric acid) was nearly the same when equal weights of the same priming composition (0.4 gram) were used and the charges subjected to similar pressures. (Tech. Paper 125, p. 59.) The dividing line between complete and partial detonation is sharp; it is possible to determine within 0.01 gram (around 5 per cent.), the weight of a detonating agent necessary to detonate completely 0.4 gram of TNT (trinitrotoluene) or tetryl, although strong detonations sometimes occur with charges several hundredths of a gram less than the minimum charge certain to produce detonation. (Tech. Paper 162, p. 12.)

IV. THE DETONATING SYSTEM.

The detonating system in military explosive constructions is still not at all satisfactory in its action. One man in close touch with munitions manufacture during the World War has estimated that 50 per cent. of the shells made for the various belligerents were "duds," because of mechanical faults or defects of the detonating system proper; this includes the large number which were rejected on inspection and never reached the front, but does not include those which would function, but with less than maximum effectiveness. Where the detonating system was responsible, the fault frequently lay in the explosive substances, which deteriorated with time and thus became less sensitive or effective; in a great many cases the design was unsatisfactory, and failed to provide an adequate margin between operation on the one hand and partial or complete failure on the other.

An underlying cause of many of these failures is the paramount necessity for provisions against premature explosions; one premature explosion may be more serious in its results than a rather large percentage of failures. The mining squadron which laid the North Sea barrage carried over 1,500,000 pounds of trinitrotoluene on each excursion, the larger mine-layers carrying 250,000 pounds each—the accidental explosion of a single mine might have wiped out the entire force and defeated the project. As they went in and out of the base, indeed, they passed the wreck of a British mine-layer which had been blown up in just this way; among those killed were several of the British officers most experienced in mining, the loss of whose services was more serious than the loss of the ship.

Safety precautions of two kinds are usually taken; in the first place every effort, both in design and in handling, is taken to prevent accidental explosion of any member of the system; besides this, provisions are made in the design so that if premature explosion of the detonator, which is very sensitive and far more liable to be fired accidentally than the booster or the main charge, will not cause explosion of the main charge. The first kind of precaution rarely affects the design of the detonating system directly, except in so far as reduction in the size of the detonator tends to reduce the danger of an explosion. The second precaution consists essentially in the introduction of a gap or break in the detonating system between the detonator and the booster (or main charge, when no booster is used), the gap being eliminated automatically when the assembly is launched, shot from a gun, or dropped, as the case may be; the detonator must not be so large that it may transmit the explosion across the gap. In mines, torpedoes, and bombs the detonator is very often not inserted until immediately before use, and then is put in the firing position; in this case there is no limit to the size of the detonator, but there is increased danger of premature explosion of the charge after the detonator is inserted, and the necessity for quick and easy insertion may complicate the mechanical design.

The arrangement of different detonating systems differs greatly with the purpose of the construction in which they are used, the particular conditions of use for which the safety precautions must provide, the size and shape of the space available, and a number of other variables. The general action is, however, the same in all cases: the initiator, a first-order explosive, is exploded by the firing impulse; this explosion ignites the detonator charge, composed of a detonating agent, which converts the ignition into a detonation; this detonation may be used as the initial impulse for the main charge, but almost invariably acts through a booster, which, like the main charge, is an explosive of the second order.

The use of an initiator to ignite the detonator charge has no bearing on the effectiveness of the explosion, but is, nevertheless, of distinct practical advantage. It need not necessarily be a detonating agent; it is possible, therefore, to use substances which are less likely to deteriorate, or are more sensitive to the firing im-

pulse, or which have some other advantage over the detonating agent employed in the charge. In percussion firing, the initiator must be small, for increase above a given size for a fixed value of impact causes a notable proportion of misfires. In delayed action and time fuses, where a slow-burning powder train is used to cause a certain time to elapse between the firing impulse and detonation, an initiator separate from the detonator charge is obviously necessary. In very small charges, however, the initiator may be eliminated, and the detonator receive the firing impulse directly; in such cases the booster is also omitted. The initiator and the detonator charge are always assembled in the same unit—in shell they are contained in the fuse, and in torpedoes, mines, and bombs they are placed in a single container to form the "detonator."

The principal reason for use of a booster is to reduce the amount of detonating agent required, and thus to decrease the danger of a premature explosion. High densities of loading, with a consequent decrease in sensitiveness, are habitually used in main charges in order to obtain the maximum explosive effect. Instead of increasing the power of the detonator sufficiently to overcome this decrease in sensitiveness, the detonator is made to act through a booster of somewhat greater sensitiveness than the main charge. The manner in which the booster acts can best be understood by taking a case where the same nitro-aromatic explosive is used in both booster and main charge. Assuming that the detonator gives an impulse slightly less than that required to detonate the main charge, no advantage will result from using a booster of the same density of loading; as the density of loading is decreased, the thickness of the layer decomposed increases, rapidly at first, but more slowly as the distance of the farther side of the layer approaches the rather small maximum distance at which the impulse is able to effect any decomposition; the amount of explosive in unit thickness of layer decreases directly with the density of loading; there is, then, a given density at which the amount of explosive decomposed by the impulse is a maximum. The velocity of propagation through the booster charge, and therefore its effectiveness, also decreases with decreased density; the most effective density for the booster, therefore, is somewhat above that at which the initial impulse causes directly decomposition of the largest amount of the booster. This value for new

substances is at present determined by "cut-and-try" methods; it appears to be dependent upon the state of division of the explosive, and perhaps other factors, as well as upon the booster material itself.

The nitro-aromatic explosives are the only substances now used for boosters; in former years, when guncotton was the only high explosive used for military purposes, it was customary to add a large proportion of water to the main charge to decrease its sensitiveness, and to use a booster (or "primer," as it was then called) of dry guncotton to ensure detonation; now nitro-aromatic explosives are used even here. Tetryl and TNT are the ones most often used, tetryl being generally preferred. It is easier to detonate than TNT, although far from being dangerously sensitive, and is more powerful; a distinct advantage is that moisture in a fulminate of mercury detonator, a small amount of which will prevent its detonating TNT, has considerably less effect upon its effectiveness with tetryl. Tetryl, on the other hand, is considerably more expensive than TNT, and is less stable, especially when not carefully purified. Several other explosives of this class, picric acid in particular, also find an extensive use in boosters. Whatever the substance employed, it is used in a finely-divided state, thereby still further increasing its sensitiveness to the explosive impulse without making it more sensitive to shock. The booster material is always of a high degree of purity in order to obtain the best possible power, stability, and uniformity.

No increase in explosive effect seems to result from the use of a booster, except perhaps for the slight improvement obtained by better distribution of the explosive impulse through the main charge. The results will be practically the same if the detonator is increased to a suitable size and made to act directly upon the main charge; in blasting work, in fact, where the size of the detonator is not limited by considerations of safety, boosters are rarely used. The booster in shell is often assembled in one with the fuse; in mines, torpedoes, and bombs the booster is a separate unit.

No general rules for the size of detonator or of booster necessary to accomplish detonation can be given. The weight of the primary charge (the one which furnishes the impulse) for best results is dependent not only upon the chemical and physical

characteristics of both explosives, but also upon many other factors, including the shape of both charges, their location with reference to each other, the design of containers, and especially the distance between the two charges and the medium filling the gap. It is a question whether increase in size of the primary charge above that just sufficient to produce detonation has any influence upon the effectiveness of the explosion. Waddell believes that increase of impulse above the minimum does result in a certain degree of increased effectiveness, even when the secondary charge is of a more powerful explosive than the primary—in other words, that it requires an impulse somewhat larger than that just able to produce detonation to bring out the full power of a second order explosive (private communication); the difference is far smaller, of course, than that between a high order and a low order explosion. Everything considered, the chief point to be considered in determining the size of the detonator and the booster, as far as explosion of the main charge is concerned, is to make them both powerful enough to provide the excess of energy necessary to make the action certain. It may not be amiss to point out, in this regard, that while great power on the part of the detonator will tend to make up for a weak booster, no booster, however large or powerful, can function when the detonator is too weak.

The detonator, in particular, since its power is limited, must be so located that the largest possible proportion of its explosive energy will act upon the material of the booster. The explosive wave, if unhindered, proceeds radially from the exploding substance, and only that portion of it which impinges upon the secondary charge is useful in transmitting the explosive impulse. This point is of importance with boosters only as far as questions of space, assembly, and cost limit the size. Confinement of the explosive wave so that, to escape, it must all traverse some part of the secondary explosive increases the efficiency with which the energy of the primary charge is used; in the long cylindrical detonators which are in common use, the confinement exercised by the shell results in a large proportion of the impulse radiating out from a point at or near the end opposite the initiator; by burying this end of the detonator in the booster material most of the energy of the detonator will be forced to act on the booster.

The location of the detonator and booster, it may be said

in passing, exerts some influence upon the effectiveness of the explosion. The speed of decomposition will obviously be best when the impulse upon the main charge acts as if it were proceeding from the centre of gravity of the charge. The distribution of the explosive effect of the charge in different directions is also dependent upon the part decomposed by the initial impulse as well as on the shape of the charge itself, on the design of the container, and probably on other factors; other things being equal, the tendency seems to be for the effect to be at a maximum on the side farthest from the impulse, but no complete study of the matter has as yet been made. The distribution of the primary charge also may be of importance; for example, in a long, slender charge of an explosive of which the rate of detonation is less than that of the booster material, the speed of explosion may be built up practically to that of the booster by having the latter extend the full length of the charge; at the same time, if this explosive be so insensitive that the detonation tends to drop to a deflagration within a short distance, the long booster will improve certainty of action.

A distinct loss of effectiveness of the explosive impulse results from air-gaps between the different charges of the explosive system—the loss is so great that a small air-gap (of an inch or less) between the detonator and the booster is an excellent means of preventing transmission of an accidental explosion of the detonator to the booster. Where a gap exists when the system is in the "armed" or firing position, a considerable increase in the size of the primary charge is necessary, for the radius of effectiveness varies only as the square root of the primary charge, if not even more slowly. As a result of limitations in design, or of poor functioning of the arming mechanism, air-gaps are sometimes found between the detonator and the booster. Air-gaps between the booster and the main charge may result from shrinkage of a cast charge leaving cavities around the booster, or, in shell where the booster does not extend down into the charge, from the charge moving bodily away from the booster on firing, when a nose fuse is used, or on impact in the case of a base fuse. The cavitation difficulty may be eliminated by care in casting; shifting of the charge is to be prevented by use of a sufficiently high density of loading. The detonator and booster cases also reduce the effectiveness of the impulse, but

to a much less degree, for denser substances are better mediums for transmission of the effect; the specific ability of a medium to transmit the impulse is probably a function of the velocity of sound through it.

Two or more complete detonating systems are sometimes installed, both being fired simultaneously. This method results not only in greater certainty of action, but, particularly in long charges when a system is placed at either end, in more rapid decomposition, and greater explosive effect. Extra detonators are occasionally distributed through long charges, with the idea of increasing explosive effect and of ensuring complete detonation; these detonators take their firing impulse from the main charge. The advantages obtained are probably not great. (For tests in blasting explosives, see Bulletin 59, p. 68.) Two boosters in series are sometimes used, but usually only for convenience in assembly, to avoid the use of a single booster of a shape awkward to handle or for which the case would be expensive, or for some similar reason. As might be expected, no advantage in explosive effect results from dividing the booster charge into two parts.

The fact that a very slight decrease in the effectiveness of a detonating system just capable of always producing a detonation will result in a system which is never able to bring about anything more than a deflagration, is a point of considerable practical importance in view of our present inability to measure the properties upon which effectiveness of the explosive impulse depends. A newly-designed detonating system may operate in an entirely satisfactory manner in tests, and yet the margin of positive operation may be so small that a large percentage of partial or even complete failures will occur in service. In one case during the World War, a new design which had given detonation on every firing test preliminary to adoption, and which gave every promise of good results in service, failed badly; it was only by making the action of the detonating system more positive that it could be used at all. In another, the inventor of a new construction, finding that the detonator used was wholly incapable of detonating trinitrotoluene across the air-gap which separated them in his design, substituted tetryl for trinitrotoluene and obtained detonation; this he considered satisfactory, although it is obvious that the surplus of power available for taking care of the varia-

tions sure to come up in service was not large, and may have been very small.

One of the best ways at present available for determining the operating margin is by lowering the efficiency of the detonating system until the minimum impulse which will produce detonation is found, or until the margin has been proved to be sufficient. This is done better by changes in the explosive materials used than by decreasing the weight of the detonating charge and the booster, for the latter inevitably involves alterations of distribution, of size of air-gaps, of densities of loading, and other important conditions. For instance, suppose the system as designed has a detonator charge of an 80/20 mercury fulminate-potassium chlorate mixture, and a tetryl booster. The effectiveness of the detonator can be reduced by substituting the same weight of "straight" fulminate of mercury for the fulminate-chlorate mixture; similarly, the sensitiveness and the power of the booster can be lowered by the use of TNT instead of tetryl. The detonating system described should, in the opinion of the author, operate satisfactorily under these conditions if it has a satisfactory excess of effectiveness over that barely necessary. If it is desirable to carry the test still further, there can be mixed with the fulminate other substances—oxides of the heavy metals which reduce only to lower oxides and not to the metals themselves, for example—which reduce but do not prevent effectiveness. (See Tech. Paper 162, pp. 13 and 14, for the effect of certain substances upon the efficiency of fulminate of mercury.) At the same time, the sensitiveness and effectiveness of the booster can be lowered still further by mixing with the TNT some such explosive as dinitrotoluene or dinitrobenzene. Whether this or any other test is used, the effectiveness of the detonator in its action upon the booster as well as the effectiveness of the entire system should be determined, as malfunctioning between the detonator and the booster is perhaps the commonest fault in the system.

Direct tests of detonators can be made by the sand test described by Storm and Cope. (Tech. Paper 125; description also appears in Colver, p. 554. For comments, see Tech. Paper 162, pp. 19–21 and p. 23.) It appears possible to develop this method to take care of the entire detonating system, although it is a measure of strength rather than of detonating ability, and

therefore not adapted to give direct comparisons of substances differing widely from each other. The sand test will also be of some value in connection with the method described in the previous paragraph, for obtaining comparisons between the materials specified and those used in the tests.

V. INITIATING COMPOSITIONS.

The value of a primary ignition agent, it is seen from the foregoing, depends upon its ability to develop the heat necessary to ignite another explosive; similarly, the value of a primary detonating agent is dependent upon its exploding in the particular manner which will result in the detonation of a secondary charge; in either case, the agent must be sensitive enough to be exploded by a rather weak initial impulse, given by the direct application of heat or by a blow, but must not be so sensitive as to be liable to accidental explosion in necessary handling or in use. From the practical standpoint, the ability to retain these qualities unchanged is equally essential; what we are interested in is the sensitiveness and effectiveness when the composition is used, not what they were when it was first prepared.

It is obvious from the use of these substances that they are particularly liable to deterioration. They are valuable for the purpose of initiation because they are unstable; in other words, the atoms composing the system will rearrange themselves to form other compounds upon slight provocation. In an explosion, this rearrangement takes place with great rapidity and with the evolution of heat and of gases in large quantities; under conditions often prevailing in storage and in service, however, it may take place slowly and give out much less heat, and the products of the reaction may all be solids or liquids. A rearrangement of the latter kind may involve only a single substance, its molecules passing over into a new compound having the same percentage composition as the old, but a different configuration and therefore different properties, or it may result from the interaction of the different substances of the composition with each other or with the container or its coating if it has been painted or varnished; whatever takes place, the new substances formed may react on those still unaltered. Temperature has a decided influence upon the rate with which the action progresses; most reactions are doubled in velocity by a rise of about 10° C. in tem-

perature; compositions which are satisfactory for use in a cold climate may, therefore, prove useless in the tropics. Deterioration, it must be understood, is not a fault peculiar to the initiating agents—the explosives of the second order, which are very stable in comparison with these sensitive substances, very often show changed sensitiveness and effectiveness after being stored for some years. The usual course of deterioration is toward decreased sensitiveness and effectiveness, although products more sensitive than the original composition sometimes result; in any case, heat is evolved, and, unless it is carried away, may increase the temperature above the ignition point of the system.

A condition contributing directly to deterioration, and at the same time directly causing decreased sensitiveness and effectiveness, is the presence of moisture in the initiator composition. If the system could be made and kept absolutely dry, it would for the most part retain its properties indefinitely. Efforts are made to exclude moisture, but a certain amount (often quite sufficient to start deterioration) is always taken up from the air between removal from the driers and final sealing; it is impossible, too, to make the sealing air-tight in every case, and, where leaks occur, moisture is drawn from the air until equilibrium is established. The effect of moisture on the explosive qualities, and the chemical behavior of the constituents in the presence of small amounts of moisture must be given great weight in the choice of initiating compositions.

The size of the particles which compose the initiating agent has considerable bearing upon sensitiveness and rapidity of explosion. In the case of compounds which are in themselves detonating agents, such as fulminate of mercury, it has been repeatedly demonstrated that sensitiveness to impact increases with size of crystals, although the ignition point remains the same. The opposite is true where the system is a mixture of substances which alone are fairly stable; in this case the more intimate mixture resulting from finer division tends toward increased sensitiveness. The time required for the explosive reaction is always less with the finer material, because of the increased surface; the increased effect obtained from very finely divided fulminate of mercury by Wöhler can only be ascribed to more rapid action; it has also been found that the time taken by small-arms initiators to act was much more regular when the sizes of the components

were more closely controlled than is ordinarily thought necessary.

The greater exposed surface resulting from decreased size of particles permits deterioration to proceed more rapidly, and increases the amount of moisture which a given weight of composition can absorb. The volume occupied by a given weight of initiation charge under a given pressure is greater with small particles,[2] thus making necessary an alteration of either weight, loading density, or volume, sometimes at the expense of efficiency. The explosion of a very finely-divided composition may be altogether too rapid and violent for good results, and it is impossible to maintain any control of the exposed surface when the ingredients pass the 200-mesh sieve, the finest which can be practically used. (The total surface of a given weight of a substance consisting wholly of particles of the same size and shape increases in inverse ratio to any given linear dimensions, as length of the particles. See "Air-Elutriation of Fine Powders," by A. S. Cushman and Prevost Hubbard, *Journal of the American Chemical Society*, April, 1907, p. 594.) Finally, it is unsafe to grind many of the compounds used, and it is a matter of considerable difficulty and increased expense to make them separate out of solution in a state of very fine division. Everything considered, the size usually taken is the smallest practically obtainable under good manufacturing conditions without the use of expensive additional operations.

Sensitiveness and rapidity of explosion are also dependent upon density of loading. The sensitiveness of practically all compositions, whether single compounds or mixtures, decreases continually with increased density, until in some cases the sub-

[2] This phenomenon is not generally recognized. The writer, following Cushman, has found the volume occupied by 10 grams of sand, sieved out of the same lot, and settled by tapping on the table, to be as follows (volumes accurate to 0.3 c.c. only):

Mesh	Volume
60–80	6.2
80–100	7.5
100–200	8.0
Past 200	8.5

Cushman has found it impossible to reduce the volume of the finely divided material to that of the coarser even with high pressures. For discussion, see A. S. Cushman, "On the Cause of the Cementing Value of Rock Powders," *Journal of the American Chemical Society*, May, 1903, p. 459.

stance is " dead-pressed " and will not explode at all; the sensitiveness of lead azide, to flame at least, seems to be practically independent of the density. The effect of density upon the velocity at which the explosion proceeds depends upon the composition itself; if it consists wholly or in large measure of a compound explosive in itself, the velocity usually increases with increased density; in the case of mixtures of substances not in themselves sensitive explosives, it increases to a maximum and then drops off. Reliable quantitative data upon this effect is not available because of the difficulty of measuring the velocity of propagation through these substances, but this is in accordance both with observations of the effectiveness of various compositions at different densities, and with the behavior of the less sensitive explosives.

Fulminate of mercury is by far the most important of the substances used in initiating compositions. It is the chief component of every detonating composition in common use, and, either alone or mixed with other substances, is found in a large number of the initiators proper, particularly those for percussion firing. Its relative importance is gradually becoming less, to be sure; non-fulminate compositions are coming to be used more and more for initiation in primers and, since 1907, when Wöhler and Matter announced the existence of more effective detonating agents, it has no longer been true that without it the valuable organic explosives could not be used.

Fulminate of mercury results from the action of alcohol upon a solution of mercury in an excess of nitric acid. It is a white crystalline substance; commercial fulminate, however, frequently has a grayish or brownish cast due to traces of resinous substances formed during the reaction, or possibly to enclosed particles of finely-divided mercury. It is very slightly soluble in water, somewhat more so in alcohol, and very soluble in pyridine and in aqueous potassium cyanide; the latter two solvents may be used for purification if desired, but it is usual to use the compound as originally formed, after thorough washing with cold water to remove all traces of acid, and drying.

The specific gravity of fulminate is 4.4; when merely tapped down, however, its apparent density is only about 1.7, the density at which it is usually used is around 2.2, corresponding to a loading pressure of 250 kilograms per square centimetre, or 3500 pounds per square inch. (Marshall, p. 512.) If subjected to

great pressure, it cannot be exploded by flame, but will apparently detonate with excellent effect if subjected to the impulse obtained from another detonation. When heated, even unconfined, it detonates at from 150° C. to 200° C., depending on the rapidity of heating; the violence of its action is greatly increased by slight confinement. The sensitiveness to impact is dependent on the size of crystals and upon experimental conditions; Will found the blow given by a 2-kilogram weight falling 2 centimetres to be sufficient. (Brunswig, p. 26.) It is, therefore, too sensitive as well as too expensive for use as a main charge—its only employment is in the initiating system.

Fulminate of mercury, as well as the compositions of which it is a constituent, has several undesirable qualities. It deteriorates rather easily, especially in the presence of moisture; in Austria there is an official regulation that detonators are to be stored over fused calcium chloride. Temperatures such as those often encountered in the tropics are of themselves sufficient to produce rather loss of effectiveness, apparently even in the absence of moisture. A moisture content of one per cent. is usually enough to make it ineffective, the explosive qualities being regenerated again upon drying; Storm and Cope found, however, that 0.75 per cent. moisture in 80/20 mercury fulminate-potassium chlorate detonator charges weighing 1.44 grams, caused a negligible reduction in strength as shown by the sand test; in the case of detonator charges consisting of the same fulminate-chlorate mixture superposed on an equal weight of TNT, exposure to an atmosphere saturated with moisture at laboratory temperature resulted in incomplete detonation of the combined charge. (Tech. Paper 125, p. 61.) Mercury fulminate attacks both copper and brass in the presence of moisture, cuprous fulminate, together with zinc fulminate in the case of brass, is formed, and metallic mercury is set free;[3] the action is sometimes sufficient to eat through thin containers, and, if the brass has not been properly annealed, cracking will result. The new fulminates formed are in themselves brisant explosives, nearly equal in effectiveness to mercury fulminate itself, but they are less stable, and are considered by some to be dangerously sensitive. Finally, mercury fulminate is very dangerous to handle and is expensive.

[3] If mercury fulminate and either zinc or copper are treated with boiling water, metallic mercury is set free, and zinc or copper fulminate, as the case may be, is formed.

Lots of fulminate of mercury obtained from different manufacturers, and even different lots obtained from the same plant, differ materially in their liability to deteriorate; this matter does not seem to have been studied thoroughly. (Marshall, p. 511.) The differences are such as not to be detected in the methods of inspection now in use. Some explosive chemists of to-day believe, in fact, that Howard, who discovered this compound in 1799 and was the first to describe a method for its production, consistently made better fulminate than any one has been able to manufacture since.

Initiators in electric primers, and to a large extent those in electric detonators, consist of finely-divided dry guncotton, of a mixture of dry guncotton and meal powder, or of some other highly-inflammable composition. This material, being soft, is practically insensitive to shock under the conditions of use and the danger of deterioration is considerably less. Largely as a matter of manufacturing convenience, the fulminate composition used in the detonator charge may also be used in the initiator.

Mercury fulminate when used alone is too quick-acting for good results in percussion primers, whether they are for the direct ignition of the propellant or of an ignition charge. From the very first the custom has been to mix with the fulminate other substances which at once decrease the violence of the explosion and add to its heat; the percentage of fulminate used has steadily decreased as time passed, and the content in most modern mixtures does not exceed 20 per cent. (Marshall, p. 504.) Saltpeter, sulphur, and meal powder were formerly mixed with the fulminate in various proportions, and powdered glass was frequently added to increase sensitiveness, which was very likely to diminish quickly because of the hygroscopic properties of the saltpeter. At the present time, potassium chlorate and antimony sulphide (obtained by powdering the sulphide occurring naturally as "stybnite," after purification by liquation if necessary) are used almost exclusively; in these mixtures, antimony sulphide acts as a fuel, the chlorate furnishes the oxygen necessary for its combustion, and the fulminate by its explosion raises the temperature high enough to start the combustion in the first place. The antimony sulphide, which comes in hard, sharp-edged particles, also contributes to the sensitiveness of the composition in the same manner as powdered glass did in earlier mixtures; the

decomposition products of antimony sulphide are soft, however, and do not erode the bore like powdered glass.

Work upon the development of primer compositions which contain no fulminate of mercury or other substance in itself explosive has been going on for a number of years. The earliest of these contain potassium chlorate and antimony sulphide, together with some other fuel which will react readily with the chlorate upon impact, and thus produce the heat necessary for combustion of the less reactive sulphide. From 10 to 35 per cent. of powdered sulphur was usually used as this second fuel. These mixtures showed a tendency to decompose gradually and the results obtained were very erratic; the output of a plant would be excellent for a time, and then, for some unaccountable reason, a markedly inferior composition would be produced. It now seems that the erratic results were due to improper methods of inspection, or even no inspection, of the ingredients, with the result that impurities fatal to stability were unknowingly introduced; Cushman has pointed out, for example, that under the methods commonly used for testing stybnite, an antimony sulphide content reported as above 98 per cent. might actually be below 80,[4] and sometimes there was no analysis of the raw materials. Since commercial potassium chlorate may contain traces of chlorate, hypochlorite, or bromate—all much less stable than the chlorate—or of the hygroscopic sodium chlorate, it is easily seen that differences in the raw materials themselves from lot to lot would readily cause variations. Even with an ideal inspection system, however, the sulphur was sure to be a source of deterioration; it is well established that finely-divided sulphur, in the presence of air and moisture, tends to oxidize with the final formation of sulphuric acid, which, of course, will attack the other constituents of the mixture and the container.

The great problem then was to find a fuel which would react readily with the chlorate as result of the heat generated by a blow, but which would be stable under the conditions to be expected in service. Of a large number of substances tried, lead sulphocyanate (or thiocyanide—the lead salt of HSCN) has proved to be the most valuable, and compositions made up

[4] "Antimony Sulphide as a Constituent in Military and Sporting Arms Primers," by A. S. Cushman, *Journal of Industrial and Engineering Chemistry*, May, 1918, p. 376.

with it have given excellent results. It decomposes, to be sure, in the presence of considerable moisture—a mixture containing it will quickly "go sour" on a hot day—but after loading and drying the sulphocyanate compositions seem to promise a long effective life. An excellent mixture of this sort is that patented and used by one of the large American companies; this has the following composition:

Potassium chlorate	53 per cent.
Antimony sulphide	17 per cent.
Lead sulphocyanate	25 per cent.
TNT	5 per cent.

A. S. Cushman has developed and patented an interesting method of controlling the ignition effect of the primer composition. (This patent has been assigned to the Government.) It is clear that, even with the closest practicable inspection of raw materials, the ignition effect of one lot of primers may vary from that of another, and that this may have a serious effect upon ballistics in the case of small-arms. By substituting for the 5 per cent. of TNT in the above composition the same weight of a mixture of litharge (lead oxide) and tetryl, the effectiveness can be adjusted within limits by changing the proportions of the two. The litharge also acts as a "sweetener," and by absorbing any acids formed by slight deterioration prevents them from attacking the constituents still unchanged. Lead sulphocyanate, for example, gives a very slight acid reaction when moistened; litmus shows its mixture with litharge to be neutral.

The initiator for percussion detonators may either be of the same composition as the detonator charge, or may be along the lines of the priming compositions just described. If there is an air-gap between the initiator and the detonator, the latter type is probably to be preferred, since more heat is generated and the initiating action continues longer, thus making for more certain action; in this case, the fulminate content usually runs higher than in the true primer compositions, in order to obtain somewhat increased sensitiveness. The non-fulminate compositions are at present not in favor for this purpose, largely because those tried in the past were not reliable after storage, and since the amount of fulminate is small as compared with the total amount in the system; it is possible that the new improved types may show sufficiently good results to overcome this prejudice.

In detonating charges, fulminate of mercury is often used alone, but manufacturers of commercial detonators have been accustomed to introduce a small percentage of potassium chlorate to reduce the quantity of the more expensive fulminate used and to get a composition running more freely in the loading machines. There was a decided difference of opinion between the various authorities as to the influence of the chlorate upon effectiveness, until, in 1916, Storm and Cope demonstrated that the addition of chlorate up to 20 per cent. steadily improved the action. (Tech. Paper 125, p. 46.) Taylor and Cope then showed that there are a number of oxidizing agents which increase the detonating ability of fulminate, principally the salts of oxyhalogen acids, and oxides and nitrates of heavy metals whose decomposition results in free metals; alkali and alkali earth nitrates, and heavy metal oxides which reduce to lower oxides are ineffective. (Tech. Paper 162, pp. 12–14.) It seems improbable, however, that potassium chlorate will be replaced in this use. The same two investigators, following Wöhler's work, also showed that small accelerating charges of lead azide and silver acetylide caused a considerable increase in the efficiency of the fulminate. (Tech. Paper 162, p. 16.)

A variation of the mercury fulminate detonator in common use in commercial work is the "reinforced" detonator, in which about half of the fulminate composition is replaced by a nitroaromatic explosive, usually tetryl or TNT; the part of the charge nearest the fuse (which fills the same function as the initiator) still consists of fulminate or a fulminate-chlorate mixture. The explosives are in contact, and are given greater confinement by means of a copper shell fitting neatly within the detonator shell and pressed on the charge; this results in increased efficiency of the fulminate, because it attains its maximum velocity of detonation more quickly, and more of its energy is compelled to act upon the second order explosive. (Tech. Paper 125, p. 47.) This type of detonator is somewhat more powerful, size for size, than the ordinary kind. There may be more danger of deterioration because of the contact between the two explosives; the usual deterioration of the fulminate may also result in the detonator becoming ineffective more quickly, because of the small amount of fulminate used. (Marshall, p. 506.)

Lead azide [5] appears likely to replace fulminate of mercury as a detonating agent. When finely divided, it is considerably less sensitive to shock than fulminate, it is far more effective as a detonating agent, its sensitiveness and effectiveness are apparently not affected seriously by moisture, at any rate not in the quantities likely to occur, and, instead of becoming "dead pressed," its effectiveness increases with density of loading up to the highest value attainable in practice, its sensitiveness to ignition remaining practically unchanged. No report upon its stability under all conditions has been published, but from the data available it seems to be much better in this respect than fulminate of mercury. Large crystals are likely to be formed, unfortunately, which can detonate on the slightest shock even under water; several accidents of this sort have already occurred. If adopted, it is probable that the reinforced detonator described above will be used, substituting lead azide for the fulminate composition. (Marshall, p. 511.) The Germans made some use of lead azide detonators during the late war, apparently with excellent results.

Little can be said about the future of the organic detonating agents; those proposed so far appear to be too sensitive to shock and too liable to deterioration of one sort or another to be of any practical value for military use. The mere fact that there are organic substances which are more effective detonating agents, weight for weight, than any of the metal compounds is nevertheless important, in that it shows that there is a possibility of finding somewhere among the organic compounds, with their wide range of properties, a detonating agent more useful than any we now know.

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[5] The lead salt of hydronitric acid, HN_3. These salts are also known as nitrides, hydronitrides, and as trinitrides.

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An excellent bibliography of the subject of detonation appears in Technical Paper 162.

Physical Constants of Aniline.—Certain physical constants of pure aniline have been determined by C. L. KNOWLES (*Jour. Ind. and Eng. Chem.*, 1920, xii, 881–883). The freezing point is 6.24° *below* zero C.; the boiling point 184.32° to 184.39° C. at a pressure of 760 mm. of mercury; the specific gravity 1.0268 (both the aniline and the water being at a temperature of 15° C.); the index of refraction 1.5850 at 20° C. The purity (in per cent.) of a sample of aniline may be calculated by multiplication of its freezing point (° C.) by the factor 1.41, and addition of the product to 108.79.

J. S. H.

Asbestos in the United States. (*U. S. Geological Survey Press Bulletin No. 461*, December, 1920.)—The United States now obtains most of its high-grade, long-fibre asbestos from Canada, but geologists of the Geological Survey hope that large deposits which will yield material of good quality may yet be found in the Western States, especially in Arizona, where asbestos of unusually long fibre and silky texture has been discovered.

In the Apache and San Carlos Indian reservations, in Arizona, asbestos is found associated with rocks known by geologists as the Apache group, which is made up of several formations. The principal deposits are in the Salt River region, where the Apache group is represented chiefly by beds of quartzite and limestone, which are at many places invaded by diabase, an igneous rock. Throughout this area much diabase has been injected into beds of limestone, and the asbestos is found near the contact of the limestone with the diabase. Places where the limestone has been much broken by the diabase have been particularly favorable for the formation of asbestos. The asbestos is invariably associated with serpentine, a greenish mineral that is in some deposits in other regions mottled like a serpent's skin, and although serpentine occurs at many places without asbestos, serpentine "float" fragments of it that lie loose on the surface, having been washed out from its outcrop, are a valuable aid to the prospector for asbestos. In this region asbestos itself is also generally found as float for a considerable distance below its outcrop.

NOTES FROM THE U. S. BUREAU OF STANDARDS.*

THE CARBONIZATION OF LUBRICATING OILS.[1]

[ABSTRACT.]

THE principal reason for writing this circular was to give detailed descriptions of the methods used for the determination of the "Conradson carbon residue" and the "Waters carbon" of oils used to lubricate internal combustion engines.

After brief accounts of the nature and effects of "carbon" deposits in the engine, and of the chemical nature of petroleum oils, the theories concerning the causes of the deposition of this material are discussed in connection with somewhat detailed accounts of the oxidation and cracking of petroleum oils.

The Waters and Conradson tests are described in detail, together with the apparatus employed, and brief accounts are given of a few other methods, including those in which the oil is distilled.

The circular closes with a condensed summary of several more or less controversial papers that have been published elsewhere.

THE ELECTRIC-ARC WELDING OF STEEL: THE PROPERTIES OF THE ARC-FUSED METAL.[2]

By Henry S. Rawdon, Edward C. Groesbeck and Louis Jordon.

[ABSTRACT.]

A FUSION weld is fundamentally different from all other types in that the metal of the weld is essentially a casting. The arc-fusion weld has characteristics which are peculiar to it alone. A knowledge of the mechanical properties of the arc-fused metal which is added during the process of welding is fundamental in the study of arc-welding. The mechanical properties as revealed by stressing in tension were determined upon specimens (0.505 in. diameter, 2 in. gage length) cut from blocks of arc-fused metal prepared under conditions similar to those met in welding. Additional specimens were also prepared by expert welders outside of the Bureau and submitted for comparison with those prepared by the Bureau.

Two types of electrodes, a "pure" iron and a low-carbon

* Communicated by the Director.

[1] Circular No. 99.

[2] Technologic Paper No. 179.

steel, were used. During fusion the composition changes considerably as the carbon and other elements are eliminated. The two types of electrodes become very much alike in composition. In each case a considerable percentage of nitrogen is taken up.

The mechanical properties of the arc-fused metal as measured by the tension test are essentially those of an inferior casting. The most striking feature is the low ductility of the metal. All of the specimens examined (about 70) showed evidence of unsoundness in their structure, tiny enclosed cavities, oxide inclusions, and lack of intimate union. These appear to be a necessary consequence of the method of fusion as now practiced. They determine almost entirely the mechanical properties of the metal. The observed elongation of specimens under tension is due to the combined effect of the numerous unsound spots rather than to the ductility of the metal.

The material is, however, inherently rather ductile, as may be shown by the changes produced in its microstructure by cold-bending.

A characteristic feature of the microstructure of the arc-fused metal is the presence of numerous microscopic plates within the ferrite grains. These persist in the metal upon prolonged heating, for example, 6 hours at 1000° C. in vacuo were not sufficient to remove them. The various lines of evidence available indicate that they are related to the nitrogen-content of the metal.

The microscopic examination indicates that there is but little, if any, relation between these so-called "nitride plates" and the path of rupture produced by tensional stresses. The effect of the grosser imperfections of the metal is so much greater than any possible effect of these plates in determining the mechanical properties that the conclusion appears to be warranted that this feature of the structure is a matter of relatively minor importance in ordinary arc welds.

Judged from the properties of the metal after fusion, neither type of electrode used appears to have a marked advantage over the other. The use of slight protective coatings on the electrodes does not appear to affect the mechanical properties of the arc-fused metal materially in any way. The specimens were prepared in a manner quite different from that used ordinarily in electric-arc welding and so do not justify specific recommendations concerning methods of practice in welding.

THE USE OF AMMONIUM PERSULPHATE FOR REVEALING THE MACROSTRUCTURE OF IRON AND STEEL.[3]

By Henry S. Rawdon.

[ABSTRACT.]

AMMONIUM PERSULPHATE is a widely used etching reagent for copper alloys, but the advantages of its use with iron and steel have been almost entirely overlooked. The article describes the method of application of the reagent and gives photographs to illustrate typical results obtained. Ammonium persulphate decomposes upon the addition of water, so that the action of the reagent is that of a weak acid intensified by the presence of oxygen. Chemical unhomogeneity is readily shown by the reagent, but the most characteristic feature in its action is the readiness with which it reveals the crystalline condition of the material.

RESONANCE POTENTIALS AND LOW VOLTAGE ARCS FOR METALS OF THE SECOND GROUP OF THE PERIODIC TABLE.[4]

By F. L. Mohler, Paul D. Foote and W. F. Meggers.

[ABSTRACT.]

MEASUREMENTS of potentials of melastic electron impacts have been made in vapors of mercury, cadmium, zinc, and magnesium, using a four-electrode vacuum tube. In addition to the resonance and ionization potentials observed in previous work at potentials corresponding to the spectral frequencies $1.5S - 2p_2$ and $1.5S$ respectively, a second resonance potential has been observed in each case. This corresponds to the frequency $1.5S - 2P$. A spectroscopic study of the low-voltage arc in magnesium proves the existence of the line $1.5S - 2p_2$, $\lambda = 4571\ A$ as the single-line spectrum. This is contrary to the conclusion arrived at by McLennan from a similar experiment and removes a difficulty in the theory of resonance potentials and low voltage arcs. The present work shows that the spectral relations of critical potentials are similar for all metals of the second group.

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[3] Scientific Paper No. 402.

[4] Scientific Paper No. 403.

Sulphur Dioxide as an Oxidizing Agent.—WILLIAM WARDLAW, FRANCIS H. CLEWS, and SIDNEY R. CARTER (*Jour. Chem. Soc.*, 1920, cxvii, 1093–1103, 1241–1247) have made a study of the oxidizing action of sulphur dioxide on ferrous salts. While sulphur dioxide usually acts as a reducing agent, it also possesses oxidizing powers. Thus, if a solution of ferrous chloride in 33 per cent. hydrochloric acid be treated with sulphur dioxide, the iron is oxidized to the ferric state, and the sulphur dioxide is reduced to elementary sulphur; for this solution the optimum temperature is 95° C. The oxidation does not occur at 95° C. if the solution of ferrous chloride contain less than 165 grams of free hydrogen chloride per litre. The entire ferrous iron content of the solution is not oxidized. The maximum amount of the total iron changed to the ferric state was 9.5 per cent.; this yield was obtained in a sealed tube experiment. Solutions of iron in 33 per cent. hydrochloric acid, containing from 10 to 18.3 per cent. of their total iron as ferric iron, and kept at a temperature of 115° C., showed no evidence of either oxidation or reduction when a mixture of sulphur dioxide and hydrogen chloride, containing equal quantities by weight of the two gases, was passed through the solution for several hours. Under these conditions, solutions containing more than 18.3 per cent. of their total iron in the ferric state were slowly but incompletely reduced.

Study was also made of the action of sulphur dioxide on the phosphates of iron in the presence of concentrated phosphoric acid. No evidence was obtained that sulphur dioxide can act as a reducing agent on ferric phosphate in the presence of concentrated phosphoric acid. Sulphur dioxide oxidized ferrous phosphate to ferric phosphate in the presence of concentrated phosphoric acid, even in mixtures in which the ferric salt already formed a high per cent. of the total iron. Whether all the ferrous iron may ultimately be oxidized to the ferric state is uncertain.

J. S. H.

Digestion of Starch by Diastase.—The value of a malt is determined by its power to digest starch. EDWARD I. ROSENBLUM (*Jour. Soc. Chem. Ind.*, 1920, xxxix, Trans., 311–313) finds that the amount of acid salts present in starch varies with the sample; consequently the hydrogen ion concentration of the starch varies with the sample. The ability of the malt to digest starch (diastatic power) is influenced by the hydrogen ion concentration of the medium, and is at a maximum when the medium is faintly acid to methyl red but is alkaline to methyl orange. This optimum reaction may be obtained by addition of ammonium dihydrogen phosphate; usually 0.5 per cent. of this salt is the correct amount to be added to the medium.

J. S. H.

NOTES FROM THE U. S. BUREAU OF CHEMISTRY.*

TOXICITY OF BARIUM CARBONATE TO RATS.[1]

By E. W. Schwartze.

[ABSTRACT.]

A SERIES of experiments on the toxicity of various substances to rats and their suitability for poisoning these animals was undertaken by the Bureau of Chemistry as part of the rat-extermination work of the Department of Agriculture. The results of the tests with barium compounds brought out the following facts:

Lethal Dose for Rats.—Barium chlorid, subcutaneously, 45 to 89 mg. per kilo, by stomach tube, 350 to 535 mg. per kilo; barium carbonate, per os, 630 to 750 mg. per kilo. On the basis of the barium content, the carbonate is about two-thirds as active as the chlorid when administered per os.

The average intake of food, both poisoned and unpoisoned, by hungry white rats, was one-hundredth of their body weight.

Twenty per cent. of barium carbonate in the rat bait was found to be an efficient concentration. With this percentage a rat is required to eat only one-third or three-eighths of a meal of average size, or one three-hundredth-and-twentieth to one two-hundred-and-sixty-sixth of its own weight, in order to secure the ingestion of a lethal amount. With this concentration, many of the rats die within the first 24 hours, the chief factor being the consumption of an amount larger than the minimum efficient lethal dose.

WEIGHT VARIATION OF PACKAGE FOODS.[2]

By H. Runkel.

[ABSTRACT.]

THE BUREAU OF CHEMISTRY has made a study of methods of packing foods under commercial conditions to determine the

* Communicated by the Chief of the Bureau.

[1] *U. S. Dept. Agr. Bull. 915*, issued Nov. 12, 1920.

[2] *U. S. Dept. Agr. Bull. 897*, issued Nov. 15, 1920.

amount of normal variation in weight. From information collected at 126 factories, suggested specifications for good commercial practice of hand weighing have been worked out. On the basis of these specifications and Bureau of Standards' tolerances, values have been assigned to constituent errors, and these errors combined according to Least Squares methods.

The figure obtained by this combination is termed the calculated maximum error of good commercial practice. According to observations taken in factories packing such packages, the chance of occurrence of variations greater than the calculated maximum error is one in more than one thousand. The calculated maximum errors in good commercial practice as determined in the bulletin are given in the following table:

Size of package.	For net weighing.		For gross and tare weighing.	
	On single packages.	On the average of a representative sample.	On single packages.	On the average of a representative sample.
	Ounces.	*Ounces.*	*Ounces.*	*Ounces.*
2-ounce	0.150	0.097	.....	.....
3-ounce	.150	.097	.....	.....
4-ounce	.150	.097	.....	.....
8-ounce	.150	.097	.....	.....
1-pound	.213	.106	.....	.....
2-pound	.360	.145	.....	.....
3-pound	.363	.148	.....	.....
4-pound	.409	.194	.....	.....
5-pound	.409	.209	.....	.....
10-pound	.544	.373	0.674	0.414
25-pound	5.41	2.68	7.49	3.34
50-pound	5.41	2.68	7.73	3.57
75-pound	5.65	2.92	7.73	3.57
100-pound and above	5.65	2.92	8.29	4.13

A study is also made of the variations produced by weighing.

SOME PROTEINS FROM THE MUNG BEAN, PHASEOLUS AUREUS ROXBURGH.[3]

By Carl O. Johns and Henry C. Waterman.

[ABSTRACT.]

THE mung bean contains about 21.74 per cent. of protein ($N \times 6.25$). Experiments with aqueous sodium chloride in various concentrations indicated a 5 per cent. solution as the

[3] Published in *J. Biol. Chem.*, **44** (1920), 303.

most effective extractant; it dissolved 19.0 per cent. of protein from the finely ground seed.

The 5 per cent. sodium chloride extract yielded two globulins, designated the α- and β-globulins, by fractional precipitation with ammonium sulphate and subsequent purification of the fractions. The yields were 0.35 and 5.75 per cent., respectively, computed on the basis of the dry material extracted. Traces of albumin, also, were obtained from extracts from which all the globulin had been precipitated by prolonged and repeated dialysis. The albumin remained in solution during the dialysis and was separated by slightly acidifying and coagulating at 45° C. The yield of the albumin was from 0.02 to 0.05 per cent. of the weight of the bean meal.

Analyses showed marked differences in the nitrogen and sulphur content of the three proteins. The globulins were still further distinguished from each other by marked differences in their percentages of the basic amino-acids, determined by Van Slyke's method. The β-globulin contained so little cystine that that remaining undecomposed after hydrolysis escaped precipitation by phosphotungstic acid and could not be determined by Van Slyke's method.

SOME AMINO-ACIDS FROM THE GLOBULIN OF THE COCONUT AS DETERMINED BY THE BUTYL ALCOHOL EXTRACTION METHOD OF DAKIN.[1]

By Carl O. Johns and D. Breese Jones.

[ABSTRACT.]

THE products obtained by the hydrolysis of coconut globulin by boiling with 20 per cent. hydrochloric acid for 27 hours were subjected to the butyl alcohol extraction method of H. D. Dakin.

Attention was confined chiefly to the amino-acids remaining in the aqueous solution after the extraction. The following values were found: Glutaminic acid, 19.07 per cent.; aspartic acid, 5.12 per cent.; alanine, 2.67 per cent.; and serine, 1.76 per cent. No hydroxyglutaminic acid or glycine could be detected.

From the amino-acids extracted by the butyl alcohol 5.54 per cent. of proline, and 0.64 per cent. of leucylvaline anhydride were isolated.

[1] Published in *J. Biol. Chem.*, **44** (1920), 283.

Agricultural Lime.—R. C. COLLISON of the New York Agricultural Experiment Station at Geneva states in *Bulletin 478* of that station that at least 50 to 75 per cent. of the soils of that state would be distinctly benefited by the addition of suitable basic material. The following substances are used for this purpose: Burned lime, hydrated lime, ground limestone, and lime by-products; usually the most economical is a good grade of ground limestone with a calcium carbonate equivalent of over 90 per cent. If ground limestone be applied at the rate of 500 to 1000 pounds per acre, it should all pass a 40-mesh screen; if applied at the rate of 2 or more tons per acre, it need only pass a 10-mesh screen. The smaller amount should be applied yearly, while the heavier treatment is required but once every three to five years. Magnesium limestone may be used for agricultural purposes. Limestone screenings and crusher dust (a by-product in the manufacture of crushed stone) frequently are a fair grade, cheap material suitable for use in liming soils.

J. S. H.

Alcohol as a Motor Fuel.—Preliminary researches by H. B. DIXON on the use of alcohol as a motor fuel are described in *Jour. Soc. Chem. Ind.*, 1920, xxxix, Review, 355. Alcohol was found to possess most of the properties which are characteristics of a good motor fuel. Its caloric value is lower than that of petrol, but it may be used under higher pressures than the latter. This property of high ignition temperature under compression is changed but little by admixture of 20 per cent. of benzene or of petrol. Such a mixture starts readily in the cold, and runs very smoothly in an engine.

J. S. H.

Colloids.—From a critical study of colloids, H. D. MURRAY (*Science Progress*, 1920, xv, 234–242) concludes that the deepest insight into dispersoid systems has been given by the phenomena of coagulation, and that future knowledge of colloids will probably come from that direction.

J. S. H.

Natural Deposits of Potash.—H. D. RUHM urges that search for natural deposits of soluble potash salts be made in the salt fields of New York, Ohio, and Michigan by means of core drilling. Deposits of potash salts, similar to those of Stassfurt, are likely to exist in any salt beds of considerable thickness. In a search of this nature, a saturated brine must be used for the liquid in the core drills. (*Jour. Ind. and Eng. Chemistry*, 1920, xii, 837–840.)

J. S. H.

NOTES FROM THE U. S. BUREAU OF MINES.*

STATE LAWS ON USE OF ELECTRICITY IN COAL MINES.

By L. C. Ilsley.

THIRTY of the States in the Union have coal mines, and twenty-eight have regulations or laws governing the operation of coal mines. Pennsylvania was the first to have a coal-mining law of any kind in 1869. By 1890, thirteen States had coal-mining statutes. Before 1890, electricity was little used in mines, and these early codes contain practically no provisions regulating its use. Since that date the application of electricity in coal mining has made tremendous progress and now twenty-one States make some reference in their coal-mining codes to the use of electricity. The rapidity with which electrical equipment has entered into the many phases of mining has, however, greatly exceeded the advance of regulations governing its use. Even the best electrical mining codes in force need modernizing and some States have even neglected to mention the use of electricity in their mining laws.

In general, the best way to obtain satisfactory electrical equipment for mine service, and to insure its proper installation, inspection, and maintenance, is for each coal-mining State to make and enforce fundamental rules and regulations relating to the use of electricity and electrical equipment in and about coal mines.

Bureau of Mines Technical Paper 271, "Mining Laws on Use of Electricity in Coal Mines," presents the results of a study of the different State codes.

STORAGE BATTERY LOCOMOTIVES.

By L. C. Ilsley and H. B. Brunot.

IN the past ten years the use of storage-battery locomotives in coal mines has rapidly increased. Their use tends to do away with trolley wires except on main haulage roads, hence the danger of electric shock from uncovered trolley wires is lessened

* Communicated by the Director.

and the wires need not be extended into the gaseous parts of the mine. Storage-battery locomotives are, however, a possible menace to safety. Originally they were not designed for use in gaseous places, but their use in such places has gradually increased because the danger was not generally recognized.

In 1916, the Bureau of Mines, in coöperation with manufacturers of locomotives and batteries, prepared tentative specifications covering minimum safety requirements.

As specific information on many of the points involved was necessary for intelligent revision of the specifications, the bureau began laboratory and field investigations of possible dangers from headlights, from abrasive sparks struck off by brakes and wheels, and from battery equipment, with a view to the elimination of unsafe features discovered. Also, application was made of information derived from previous work on the testing of coal-cutting equipment and miners' lamps.

In July, 1919, another conference of manufacturers was held at the Pittsburgh Experiment Station. A schedule for the establishment of a permissible list of storage-battery locomotives was prepared and after minor additions was published in August, 1919, as Schedule 15 of the Bureau of Mines.

Technical Paper 264, "Preliminary Investigations of Storage Battery Locomotives," describes the development of the specifications that led to the preparation of Schedule 15, and contains data obtained through the preliminary investigation made in order to assist manufacturers who were developing apparatus.

ANALYSES OF AIR FROM BURNING BUILDINGS.

By S. H. Katz.

THE BUREAU OF MINES is coöperating with municipal fire departments for the purpose of obtaining information regarding composition of the air in burning buildings. The gas-mask laboratory of the Bureau's experiment station at Pittsburgh sent vacuum bottles for taking samples to fire chiefs in twenty-five cities. Thus far, eleven samples have been received by the laboratory. Results of analyses and pertinent data regarding these samples are given in a paper recently issued by the bureau.

Eight of the eleven samples contained no carbon monoxide;

seven of the eight were taken above the ground level and one below ground. One sample taken above ground contained 0.03 per cent. carbon monoxide, and this quantity is not dangerous to breathe for a limited time. Of the three samples taken below ground, one contained no carbon monoxide; the other two contained 0.10 and 0.35 per cent., respectively. This indicates that in a burning building the hazard from carbon monoxide is much greater below the ground level. The cause undoubtedly lies in the poorer ventilation that is usually found in cellars or basements. Abundance of ventilation provides the necessary air for complete combustion, or if incomplete combustion does occur, the carbon monoxide generated by the fire is greatly diluted with fresh air and swept away by the current. It may be noted that the highest proportion of carbon monoxide was found in a cellar where a fire was burning among piled cases. The difficult access of air to the burning parts facilitated the formation and liberation of carbon monoxide. The smoke here was very light and no difficulty was experienced in breathing. Some of the air containing dense smoke showed little change from normal air by analysis. Thus the density of smoke or the irritation and difficulty it causes in breathing are no indication of the presence of poisonous gas. None of the samples shows important deficiencies in oxygen. It is hoped that in time more air samples from burning buildings will be analyzed in order to extend the information given.

RECOVERABLE OIL IN BY-PRODUCT SANDS AND OUTCROPS.

By A. R. Elliott.

A SURVEY of the oil districts of California has been made by the bureau in order to ascertain whether the visible masses of wasted oil-bearing sands would be a profitable source of supply. From the data collected, it is estimated that 2,359,100 barrels of oil, valued at more than $3,500,000, could be obtained from the sand piles about producing wells and from the outcroppings in the vicinity of the fields. Also, many times that amount of oil scattered over nearly the total oil-producing area, might be recovered from seepage—that is, the oil permitted to soak into the ground or spread over the surface.

The character of these wastes and possible methods of recovery are given in a recent report issued by the bureau.

STENCHES FOR DETECTING LEAKAGE OF GAS.

By S. H. Katz and V. C. Allison.

LEAKAGE in the distribution of natural gas or artificial illuminating gas is a frequent cause of loss of life and property. The addition of a stench to the gas for detecting leaks would result in a material reduction of loss by leakage, thus increasing safety and promoting economy. Blue water gas is very dangerous, as it is almost odorless and contains 35 per cent. carbon monoxide. Some German cities require by law the addition of stench to this gas. Also, natural gas is comparatively odorless, and very difficult to detect, and use of stench would give warning in time to avert many of the dangerous explosions that occur from leaking gas. Technical Paper 267 of the bureau, entitled "Stenches for Detecting Leakage of Blue Water Gas and Natural Gas," gives the properties of various stenches examined, their market prices, and methods for introducing them into gas mains.

On the Variation of Thermal Conductivity During the Fusion of Metals. SEIBEI KONNO. (*Phil. Mag.*, November, 1920.)—Hitherto there has been a paucity of experimental investigations with regard to the conduct of thermal conductivity of metals, or indeed of any substances, when the change from solid to liquid takes place. E. F. Northrup and H. Tsutsumi have found that the electrical conductivity of metals falls sharply and largely in fusion. According to the electron theory, both conductivities should follow the same course. In many other cases this has been verified. It is of interest to discover whether this relation holds true also at the melting-point. The Japanese physicists find as follows:

"1. The thermal conductivity of tin, lead, zinc, and aluminum decreases with the rise of temperature up to their melting-point.

"2. The thermal conductivity of these metals decreases abruptly during melting.

"3. The thermal conductivity of (solid) bismuth and antimony slightly decreases at first, and then increases a little.

"4. During melting the conductivity of bismuth considerably increases, and that of antimony seems to increase only slightly.

"5. The thermal conductivity of all liquid metals here investigated decreases but slightly with the rise of temperature.

"6. The above changes of thermal conductivity are similar to those of electric conductivity for the same metals."

G. F. S.

THE FRANKLIN INSTITUTE.

(Proceedings of a Special Meeting held Wednesday, December 8, 1920.)

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, December 8, 1920.

A special meeting of The Franklin Institute held in pursuance to the following notice, dated November 17, 1920:

> A special meeting of The Franklin Institute will be held on the eighth day of December, 1920, at four-fifteen o'clock, in the Hall of the Institute, for the purpose of taking action upon the bequest to The Franklin Institute, contained in the last will and codicil of the late Henry W. Bartol.

was held this afternoon at 4.15 o'clock in the Hall of the Institute.

The President, Dr. Walton Clark, stated that the purpose of the meeting was that indicated by the notice of the meeting sent to all members and recognized Mr. W. C. L. Eglin, who made the following motion:

> *Resolved*, That the bequest and trust, with all conditions pertaining thereto, contained in the last will and testament of Henry W. Bartol, deceased, dated the 22nd day of September, A. D. 1911, and the codicil thereto dated the 29th day of November, A. D. 1918, both probated in the office of the Register of Wills in and for the city and County of Philadelphia as of the ninth day of October, A. D. 1920, be and the same are hereby accepted by The Franklin Institute of the State of Pennsylvania.

This motion was seconded by Mr. E. H. Sanborn and unanimously adopted.

The following gentlemen, members of the Institute in good standing, whose signatures follow, constituting more than a quorum, were present:

W. C. L. Eglin
Kern Dodge
R. W. Lesley
Henry Howson
Edward H. Sanborn
Charles E. Brinley
Benjamin Franklin
Frederick A. Riehlé
Lawrence T. Paul
Arthur W. Lowe
Monroe B. Snyder
Howard S. Graham
L. F. Rondinella
William Otis Sawtelle
George R. Henderson
William H. Thorne
William J. Haines
Coleman Sellers, Jr.
Gellert Alleman
George A. Hoadley
Alfred W. Gibbs
Nathan Hayward
T. Broom Belfield
Jacob Y. McConnell
Richard L. Binder
Guilliam H. Clamer
Harold Calvert
Walton Forstall
Walton Clark

Adjourned.

R. B. OWENS,
Secretary.

(Proceedings of the Stated Meeting held Wednesday, December 15, 1920.)

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, December 15, 1920.

PRESIDENT DR. WALTON CLARK *in the Chair.*

Additions to membership since last report, 4.

Reports of progress were presented by the Committee on Library and the Committee on Science and the Arts.

The Committee on Nominations, appointed by the President in accordance with the resolution passed at the Stated Meeting held November 17th, last, presented the following list of nominees to be voted for at the Annual Election to be held on Wednesday, January 19, 1921:

For President (to serve one year) Walton Clark.

For Vice-president (to serve three years) Charles Day.

For Treasurer (to serve one year) Benjamin Franklin.

For Managers (to serve three years) Charles E. Bonine, Alfred W. Gibbs, Howard S. Graham, George R. Henderson, George A. Hoadley, Lawrence T. Paul, James S. Rogers, C. C. Tutwiler. On motion duly seconded the nominations were closed.

The paper of the evening on "Some Characteristics of Electron Tubes" was presented by Mr. W. C. White, Electrical Engineer, Research Laboratory, General Electric Company, Schenectady, New York. An account was given of the various uses of electron or vacuum tubes. It was shown that their flexible characteristics and variety of properties make them very useful in laboratories and for engineering tests and developments. In their use, however, for special purposes difficulties are frequently encountered which are due to the properties of the tube and are not commonly known or understood. The more unusual characteristics were described. The subject was illustrated by lantern slides.

After a brief discussion the thanks of the meeting were extended to the speaker.

Adjourned.

R. B. OWENS,
Secretary.

COMMITTEE ON SCIENCE AND THE ARTS.

(Abstract of Proceedings of the Stated Meeting held Wednesday, December 1, 1920.)

HALL OF THE INSTITUTE,
PHILADELPHIA, December 1, 1920.

MR. CHARLES PENROSE *in the Chair.*

The following reports were presented for first reading:

No. 2744: Republic Flow Meter.
Prof. L. F. Rondinella, Chairman.

No. 2764: Use of the A. C. Electro-Magnet in Surgery.
Mr. Nathan Hayward, Chairman.

No. 2765 and No. 2766 } The Franklin Medal.
Dr. Harry F. Keller, Chairman.

R. B. OWENS,
Secretary.

SECTIONS.

Section of Physics and Chemistry.—A meeting of the Section was held in the Hall of the Institute on Thursday evening, November 11th, at eight o'clock, with Dr. Gellert Alleman in the chair. The minutes of the previous meeting were approved as read.

Dr. Ulric Dahlgren, A.B., M.S., Professor of Biology in Princeton University, delivered a lecture on the "Production of Motion by Animals." Attention was paid to amoeboid motion, and to motion by means of turgidity, of cilia, and of muscle, and also to the lever systems of motion apparatus. A description was given of the various forms of crawling, swimming, walking, and flying.

The communication was discussed; a vote of thanks was extended to Prof. Dahlgren; and the meeting adjourned.

JOSEPH S. HEPBURN,
Secretary.

Section of Physics and Chemistry and Section of Photography and Microscopy.—A joint meeting of the Sections was held in the Hall of the Institute on Thursday evening, December 2nd, at eight o'clock, with Dr. George Crampton in the chair. The minutes of the previous meeting were read and approved.

C. E. Kenneth Mees, Sc.D., Director of the Research Laboratories of the Eastman Kodak Company, Rochester, New York, delivered an illustrated lecture on "The Structure of Photographic Images." The relationship between the size, number, and arrangement of the particles of silver halide in the photographic emulsion and the graininess of the image was discussed. Other factors which modify the image were also considered.

The paper was discussed at length; a vote of thanks was extended to Doctor Mees; and the meeting adjourned.

JOSEPH S. HEPBURN,
Secretary.

Section of Physics and Chemistry.—A meeting of the Section was held in the Hall of the Institute on Thursday evening, December 9th, at eight o'clock, with Dr. George A. Hoadley in the chair. The minutes of the previous meeting were approvevd as read.

John S. Shearer, B.S., Ph.D., of the Department of Physics of Cornell University, Ithaca, New York, presented a communication on "Recent Advances in the Production and Application of X-rays." The newer appa-

ratus and the technic for the production of X-rays were described. An account was given of Lane's work on diffraction by crystals, the X-ray Spectrum, and absorption of X-rays for separate wave lengths. The relation of X-rays to physical theory, to therapy, and to the industries was traced.

The communication was discussed; a vote of thanks was extended to Doctor Shearer; and the meeting adjourned.

JOSEPH S. HEPBURN,
Secretary.

MEMBERSHIP NOTES.

ELECTIONS TO MEMBERSHIP.

(Stated Meeting, Board of Managers, December 8, 1920.)

RESIDENT.

MR. JULIAN D. JONES, Sup't Telegraph and Signals, Eastern Region, Pennsylvania System, Room 152, Broad Street Station, Philadelphia, Pennsylvania.

MR. R. V. TOWNEND, Victor Talking Machine Company, Camden, New Jersey.

NON-RESIDENT.

PROF. ROBERT C. COLWELL, Department of Physics, Geneva College, Beaver Falls, Pennsylvania.

MR. TERRENCE J. FORD, 847 E. Fourth Street, Bethlehem, Pennsylvania.

CHANGES OF ADDRESS.

DR. A. D. CHAMBERS, R.F.D. No. 1, Edgemoor, Delaware.

MR. FRANCIS J. COLE, 640 South Hill Street, Pasadena, California.

MR. HOWARD T. DESHONG, No. 9, Lansdowne Avenue, Lansdowne, Pennsylvania.

MR. KERN DODGE, The Dodge Steel Company, State Road and Hellerman Street, Tacony, Philadelphia, Pennsylvania.

MR. EDWIN FRANK, care Miss Elsa J. Frank, 2300 Grand Avenue, Milwaukee, Wisconsin.

MR. FRANK GITTELSON, 28 East Mt. Vernon Place, Baltimore, Maryland.

DR. JOSEPH S. HEPBURN, Hering Laboratory, Hahnemann Medical College, Philadelphia, Pennsylvania.

DR. CARL HERING, 1317 Spruce Street, Philadelphia, Pennsylvania.

MR. FRED E. IVES, 1327 Spruce Street, Philadelphia, Pennsylvania.

MR. JONATHAN JONES, Tatanager, B-N Railway, India.

MR. LAWRENCE T. PAUL, 105 Liberty Building, Philadelphia, Pennsylvania.

MR. EMILE G. PERROT, 801 Parkway Building, Philadelphia, Pennsylvania.

MR. CARL D. PRATT, 412 West Broad Street, Tamaqua, Pennsylvania.

PROF. WALTER RAUTENSTRAUCH, 43 Exchange Place, New York City, New York.

MR. FREDERICK W. SALMON, Civil and Mechanical Engineer, Richmond, Virginia.

MR. R. JOHN TITZEL, Hamilton Apartments, Norristown, Pennsylvania.

MR. ETHAN VIALL, Barbourville, Kentucky.

MR. EARL H. WELTZ, 122 Broadway, Carney's Point, New Jersey.

NECROLOGY.

Hon. Horace L. Haldeman, 1008 Real Estate Trust Building, Philadelphia, Pennsylvania.

LIBRARY NOTES.

PURCHASES.

ANDROS, S. O.—Fuel Oil in Industry. 1919.
ARENDT, M.—Storage Batteries. 1920.
BRANT, W. W.—Galileo. 1918.
CHALMERS, T. W.—Paper Making and Its Machinery. 1920.
CRAIG, E. H.—Oil Finding. No date.
DANIELS, G. W.—Early English Cotton Industry. 1920.
FLEMING, J. A.—Wonders of Wireless Telegraphy. 1919.
HOGBEN, L. T.—Alfred Russel Wallace. 1918.
PURDY, H. F. P.—Diesel Engine Design. 1919.
SILBERSTEIN, L.—Report on the Quantum Theory of Spectra. 1920.

GIFTS.

American Iron and Steel Institute, Annual Statistical Report for 1919. New York City, New York, 1920. (From the Institute.)

Ball Engine Company, Bulletins Nos. S–16, S–20, S–22, S–30, S–36. Erie, Pennsylvavnia, 1920. (From the Company.)

Bartlett, C. O. and Snow Company, Booklet on Ideas. Cleveland, Ohio, 1920. (From the Company.)

Becker Milling Machine Company, Catalogue No. 1. Boston, Massachusetts, 1920. (From the Company.)

Black and Decker Manufacturing Company, Electric Air Compressors, Portable Electric Drills, Electric Valve Grinders. Baltimore, Maryland, 1920. (From the Company.)

Blanchard Machine Company, The Blanchard Grinder. Cambridge, Massachusetts, 1920. (From the Company.)

Blaw—Knox Company, Prudential Steel Buildings. Pittsburgh, Pennsylvania, 1920. (From the Company.)

Buffalo Forge Company, Catalogue No. 460. Buffalo, New York, 1920. (From the Company.)

Card Manufacturing Company, Catalogue No. 30. Mansfield, Massachusetts, 1920. (From the Company.)

Cement Gun Company, Incorporated, Gunite Slabs. Allentown, Pennsylvania, 1920. (From the Company.)

Chicago Bridge and Iron Works, The Water Tower. Chicago, Illinois, 1920. (From the Works.)

Colonial Supply Company, Book 877. Pittsburgh, Pennsylvania, 1920. (From the Company.)

Connecticut Agricultural Experiment Station, Report for the Year of 1919. New Haven, Connecticut, 1920. (From the Experiment Station.)

BOOK NOTICES.

DIE ZWISCHENPRODUKTE DER TEERFARBENFABRIKATION. EIN TABELLENWERK FÜR DEN PRAKTISCHEN GEBRAUCH. NACH DER PATENTLITERATUR BEARBEITET von Dr. Otto Lange. 8vo., xxiv–645 pages including index, reference list to German patents and one chart. Leipzig, Otto Spamer; paper bound, M. 135, full bound, M. 150, each plus 40 per cent. bookseller's advance.

"Admirable" is the adjective to be applied to this work, using the word in both its Shakesperian and modern sense, for examination arouses both wonder and praise. The coal-tar intermediates have a practical and a somewhat romantic interest in chemistry. The first tar color, it is true, was produced without distinct recognition of the relation of intermediate substances to the color itself, but very soon the extensive capacity of benzene, its homologues and related substances, to give rise to isomers was recognized, and the minds of chemists were turned to the problem of the structure of these molecules. The solution came out of Germany. In the middle sixties of the last centnry, Kekulé set forth his theory of the ring structure of benzene and his suggestion of a symbol for it, now the most familiar figure in our works on organic chemistry, and which was adopted as part of the insignia of the Chemical War Service of the United States during the recent war. The views of Kekulé and his co-workers were not at once accepted in the other two nations that were then most active in the chemical industries, England and France. Indeed, many prominent chemists in those countries not only did not accept the views, but they devoted themselves to active antagonism to them, and thus delayed the development of their own coal-tar industries for many years. It is depressing to recall that the first "aniline" color was made in England, but the hint was not taken, and this most important phase of chemical industry passed to another nation that fate had marked for England's greatest rival. Of course, British and French chemists and capitalists cannot be blamed for not reading the future as it has been unfolded to us within the past few years, but they might, at least, have lent more willing eyes and ears to the suggestions that came from beyond the Rhine, instead of which they made merry over what they considered to be the extravagances and hallucinations of the "land of dam'd professors" as Lord Palmerston called Germany. Theodore Weyl, in his work on the "Sanitary Relations of the Coal-tar Colors," written over a score of years ago, and which the present reviewer had the honor of presenting in English text, said in his preface, "Thanks to the coöperation of theory and practice, the coal-tar color industry of Germany has conquered the world," and this sentence expresses the real state of the case. "Die Weltgeschichte ist das Weltgericht," and the great mistakes that have been made in this field in the past, may serve as a warning to the "Allied and Associated Powers" that if they are to render themselves independent in this and other fields of industry, it will be by equal cultivation of the pure and applied phases of science. "*Fas est et ab hoste doceri*," "it is allowable to learn from the enemy" was an ancient motto fully as applicable now as it was in the days of Ovid.

Turning from these thoughts, awakened by the inspection of Dr. Lange's

book, to the book itself, it is best to give some details of its matter and form. It is a splendidly printed volume, with clear type on good paper. The enormous amount of information is made readily accessible by careful classification, largely based on an alphabetical arrangement and supplemented by an extensive index. Thirty-six hundred and thirty seven substances are described, arranged in four series, the derivatives, respectively, of benzene, naphthalene, anthracene and phenanthrene, the first-named comprising the largest number of items and the latter the smallest. Each intermediate is given with its systematic name printed in bold-faced neat type, and in most cases the structural formula also printed neatly and clearly. The reviewer desires to remark in passing that Doctor Lange knows how to write a ring symbol, which is not the case with some distinguished English-speaking and French-speaking chemists. In addition to the above data, brief statements are made as to the methods of producing the intermediates and references to the patent literature, most prominently to the DRP list, but also in many cases to American, British and French lists. In many cases, references to German periodical literature are given. The method of the book can best be exemplified by transcribing one of the items of average extent and fullness of detail, as follows:

726	**DRP. 140 133** Ann. 182, 110 Ber. 7, 1600; 29, 2599	**1-Chlor-3-nitro-4-methoxybenzol** $= C_7H_6ClNO_3 = 187,$ 192 T. 2,5-Dichlor-1-nitrobenzol + 250 T. Methylalkohol + 114,3 T. Natronlauge (40°) 5 St. unter Rückfluß kochen, den größten Teil des Alkohols abdestillieren und das Produkt aus Sprit umkrystallisieren. Blaßgelbe Prismen, Sch.-P. 94°—96°.

It will be noticed that in the nomenclature the numerical system is followed, and not the older method with prefixes; in accordance with the latter the above compound would be designated, presumably, p-chlor-methoxy-m-nitrobenzene. The Germans still use, unfortunately, the inappropriate termination "ol" for benzene and homologues. Although using not infrequently the o,m,p, signs for the position of substituting groups in the benzene series, Doctor Lange has practically discarded the alpha and beta prefixes in the naphthalene groups, using a strictly numerical system. Thus, the substance commonly designated in English text-books as alpha-naphthol, is here designated as 1-naphthol, and substitutions in the beta-position are termed 2-naphthalene derivatives. There seems to be some irregularity in this system, for an iodo-naphthalene, the structural formula of which shows it to be of the alpha (1) type, is termed "Jodnaphthalin," though it is stated in the text that the process used for its production gives rise to a large amount of the 1-form with a small proportion of the 2-form. Still, it seems to the reviewer to be a mistake to give a structural formula without the strictly systematic name. In the case of naphthalene, the abandonment of the signs alpha and beta, and the substitution of the common numerals is an advantage to author, compositor and reader, and chemists may well say "for this relief much thanks."

It scarcely needs to be added that the book is a most important and valuable contribution to the field of coal-tar chemistry and will be an indispensable guide to all who are engaged in either the practical or theoretical work in the field which, in the advertisement of one of the largest American dye-manufacturing companies, is called the "keystone of American industries." Great economic problems are now before the nations, one of the most important phases being the building up of the coal-tar utilizations, so that each nation may be fairly independent of all others in the productions of dyes, explosives, medicines and the more than a thousand and one other substances which have been obtained from the distillation of crude fuels. Certainly, the United States does not want to be caught again as it was in 1914, without full supplies of all these types of materials. An inspection of this book will show that whatever tariff and license systems may accomplish, an equal cultivation of theory and practice is essential to the establishment of industries that can stand alone and develop independently of other nations, and that in the last analysis of such matters the scientist will regard neither friend nor foe, past, present or possibly future, but will ask merely for trustworthy data and interpretive theories.

HENRY LEFFMANN.

LABORATORY EXPERIMENTS IN ORGANIC CHEMISTRY, DESIGNED ESPECIALLY FOR USE WITH STODDARD'S INTRODUCTION TO ORGANIC CHEMISTRY. By E. P. Cook, A.M., Associate Professor of Chemistry in Smith College. Second edition, small 8vo., 70 pages, contents and index. Philadelphia, P. Blakiston's Son & Co., 1920. $1.00.

The author states that the procedures selected for presentation in this book are such as have seemed, in a large experience, most suitable for a first course in organic chemistry. The time given to the laboratory work in this course at Smith College is four hours per week during a college year. Some processes which require considerable time, are distinguished by an asterisk, that they may be omitted if necessary.

A commendable feature is the omission of all equations, thus leaving to the student the task of determining the nature of the reactions by consulting text-books. The nomenclature used is also to be commended, as it is systematic and correct. It is exasperating to see in modern chemical works or journal articles such expressions as "benzol," "permanganate of potash," "caustic soda." The book is of convenient size and shape, and the text carefully prepared.

HENRY LEFFMANN.

THE HANDBOOK OF INDUSTRIAL OIL ENGINEERING. Lubrication and Industrial Oil Section. By John Rome Battle, B.Sc. in M.E., M.E. Small 8vo., limp cloth, 1065 pages, index and many illustrations, diagrams and specimen sheets. Philadelphia, J. B. Lippincott Co., $10.00.

"Pouring oil on troubled waters" has been for a long while an expression of peace-making, but times are changed and we are changed with them, for today oil is seriously troubling the waters of internationalism. The book in hand does not deal with the economic or diplomatic problems, but with the

application of oil to the many varieties of industries. It is impossible in the space available to present more than an outline of its character and contents. It contains a vast amount of information, being encyclopædic in scope, within the field to which it is devoted. It must represent an enormous amount of labor in searching the literature, compiling and condensing data, and last, but not least, the reading of the proof. Though not stated on the title page, the book is a second edition, as indicated by the copyright entries, the first issue having been copyrighted in 1916. Each copy is accompanied by an unstamped card provided for advising the author of errors discovered by those who use the work. This is an excellent plan, and it is to be hoped that the example will be followed, inasmuch as the author promises to communicate to those who notify him of errors a summary of those that have been indicated by others. This method of keeping in touch with one's readers ought to be popular, but an experience along the same line by the present reviewer, who adopted it in a book published by him, showed that but very few availed themselves of the opportunity, for though hundreds of copies of the book have been sold, only a score or so have taken advantage of the opportunity.

As might be expected, the bulk of the book is devoted to the petroleum oils, but about two score pages are given to the fixed oils used in lubrication, which section contains a very large amount of information. In the initial paragraph, the reviewer notes a slight typographic error, the name of the radicle "glyceryl" being spelled "glyceril," but a more serious error is the statement that the radicle of glycerin or glycerol is $(C_3H_5(OH)_2)$. There is some accidental confusion here. It is probable that the author intended to write "formula" instead of "radicle." It is gratifying to see the correct spelling of the word "radicle," so often incorrectly spelled "radical," which is the adjective form.

The work is divided into 11 sections, the limits of which are indicated by thumb-pieces, but while there is a table of contents (placed a little awkwardly on the fly-leaves), there is no summary of the titles of the sections themselves, which seems to be a slight defect.

The descriptive matter is preceded by some elementary mathematics, including methods of arithmetic relating to fractions, and a brief presentation of the principles of logarithms, with a two page table of five-figure logarithms, but it is doubtful if any one who does not understand the use of such a table could gain much from what is there given.

The printing and other mechanical features of the book are excellent, and reflect credit on all concerned. The author's painstaking efforts have resulted in a most valuable compendium in a most important field of industry.

HENRY LEFFMANN.

CHEMISTRY AND CIVILIZATION. By Allerton S. Cushman, A.M., Ph.D., Director, Institute of Industrial Research. Ex-Lieut. Col. Ordnance Dept., U. S. A. Small 8vo., 148 pages, index, six plates and two illustrations in the text. Boston, Richard G. Badger. $2.50 net.

Doctor Cushman delivered the 1920 course of lectures with the above title at the Wagner Free Institute of Science, under the Richard B. Westbrook Foundation, and has presented the substance of the course with numerous additions in the book in hand. It is a very valuable and interesting essay. English-

speaking chemists are now actively engaged in bringing to the notice of the mass of the people the value of both pure and applied chemistry, and Doctor Cushman's book will be no small aid in the propaganda. Beginning with a summary of the development of the science from its earliest period to the establishment of accurate methods of procedure, the author passes to the service of the science to the human race, starting with the researches of the early nineteenth century, certain distinguished leaders being especially noted. The application of chemistry to industry is then taken up, several of the key industries being noted, namely, the alkali industry (Leblanc to Solvay), sulphuric acid, the Bessemer and Siemens-Martin processes, the discovery of benzene and the introduction by Kekulé of the benzene-ring theory, with the subsequent extensive and valuable development of synthetic chemistry.

"Chemistry and War," the subject matter of the fourth chapter, covers the topics of nitrogen fixation, coal-tar industries, poison gases and gas masks, sources of potash and chemical developments in military surgery. Concerning the first topic, one of the most important problems in applied chemistry, the book gives a concise and highly informing account. A footnote describes in some detail the work accomplished by German chemists at the plant of the BASF during the war. In this connection attention may be called to the fact that in the Emil Fischer memorial address before the (British) Chemical Society, the speaker stated that if the Germans had not had available the Haber process for utilization of atmospheric nitrogen, the war would have closed in the summer of 1915, for the British blockade would have prevented the Central Empire from getting supplies of Chili nitrate. Doctor Cushman points the moral of this fact in its relation to the development of the nitrogen fixation industries in the country, and his book will render most valuable service in this regard.

A chapter on "Chemistry and the Future" presents interesting data in regard to radio-activity and, of course, the author wrestles a little with the Einstein theory.

The last chapter discusses some of the modern aspects of chemistry, including colloids, the future of alcohol, chemical actions at high and low temperatures and pressures, the low motor-fuel problem and the story of helium. The last item naturally leads the reviewer to examine if the author has been caught in the bewitchment of the bibliography of this element, and finds it stated that Lockyer and Janssen had mapped the yellow line of the element in the sun in 1865, and had called it helium. The now famous eclipse occurred in 1868, and there is no evidence that Janssen had any share in coining the name, at least, Lockyer said in 1894, that he coined the name for "laboratory purposes."

Dr. Cushman's book deserves to be widely read among the intelligent members of the community who though not trained in chemistry are interested in the devlopments of our chemical industries. The literary style is excellent. The reviewer is glad to see that the American spelling of chemical terms is adopted.

HENRY LEFFMANN.

NATIONAL ADVISORY COMMITTEE ON AERONAUTICS.

Report No. 94. The efficiency of small bearings in instruments of the type used in aircraft by F. H. Norton, aerodynamical laboratory, National Advisory Committee for Aeronautics, Langley Field, Va. 10 pages, illustrations, plates, quarto. Washington, Government Printing Office, 1920. This report deals with the construction and properties of bearings and pivots for use in instruments. The static and running friction for both thrust and radial loads was determined for a number of conical pivots and cylindrical and ball bearings. The static rocking friction was also measured for several conical and ball bearings under a heavy load, especially to determine their suitability for use in an N. P. L. type wind tunnel balance. In constructing conical pivots and sockets it was found that the pivots should be hardened and highly polished, preferably with a revolving lap, and that the sockets should be made by punching with a hardened and polished punch. It was found that for a light load the conical pivots give less friction than any other type and their wearing qualities when hardened are excellent. When the load exceeds about 1000 grams ball bearings give less friction than pivots and will stand shocks and wear better. Very small ball bearings are unsatisfactory because the proportional accuracy of the balls and races is not high enough to insure smooth running. For rocking pivots under heavy loads it was found that a ball and socket bearing, consisting of a hemispherical socket and a sphere of smaller diameter concentric with it with a row of small balls resting between the two was superior to a pivot resting in a socket. It was found that vibration such as occurs in an airplane will greatly reduce the static friction of a pivot or bearing, in some cases to as little as one-twentieth of its static value.

PUBLICATIONS RECEIVED.

National Advisory Committee for Aeronoutics: Fifth annual report, 1919. 870 pages, illustrations plates, diagrams, quarto. Washington, Government Printing Office, 1920. Sixth annual report, 1920. 56 pages, quarto. Washington, Government Printing Office, 1920. Technical Notes, No. 17, Italian and French experiments on wind tunnels, by Wm. Knight, 8 pages, plates, quarto. Technical Notes, No. 19. The steadiness factor in engine sets, by W. Margoulis, aerodynamical expert. Paris Office, N.A.C.A. 10 pages, plates, quarto. Technical Notes, No. 24. Development of the inflow theory of the propeller, by A. Betz. Résumé translated from the German, by Paris Office, N.A.C.A. 4 pages, plates, quarto. Technical Notes, No. 27. Instrument for measuring engine clearance volumes, by S. W. Sparrow, automotive power plants section, Bureau of Standards. 4 pages, plates quarto.

U. S. Weather Bureau: Daily river stages at river gage stations on the principal rivers of the United States. Vol. 17, for the year 1919, by Alfred J. Henry, meteorologist. 291 pages, quarto. Washington, Government Printing Office, 1920.

U. S. Bureau of Mines: Monthly statement of coal mine fatalities in the United States August and September, 1920, by W. W. Adams, 2 pamphlets, 8vo. Washington, Government Printing Office, 1920.

Catalysis in the Hydrolysis of Esters by Infra-red Radiation.—ERIC K. RIDEAL and JAMES A. HAWKINS (*Jour. the Chem. Soc.*, 1920, cxvii, 1288–1296) find that the hydrolysis of methyl acetate by dilute hydrochloric acid is catalytically accelerated by infra-red radiation. The temperature of the mixture of ester and dilute acid was kept constant by means of a thermostat. In the experiment proper the mixture was subjected to infra-red radiation usually obtained from sunlight or from a glowing Nernst filament by means of an optical system of lenses and a quartz prism. In the control experiment the mixture was not illuminated. In a series of such experiments the illuminated ester was hydrolyzed far more rapidly than the ester which was kept at the same temperature in the dark.

J. S. H.

Amino Acids of Gelatin.—The amino acids which occur in the protein gelatin have been separated and determined by H. D. DAKIN (*Jour. of Biol. Chem.*, 1920, xliv, 499–529). The per cent. of each amino acid found was: Glycine, 25.5; alanine, 8.7; leucine, 7.1; serine, 0.4; phenylalanine, 1.4; tyrosine, 0.01; proline, 9.5; hydroxyproline, 14.1; aspartic acid, 3.4; glutamic acid, 5.8; histidine, 0.9; arginine, 8.2; lysine, 5.9; ammonia, 0.4. The sum of these ingredients amounts to 91.31 per cent., while previous analysts have accounted for considerably less of the total amino acid content of the gelatin, their sums ranging from 42.1 to 70.7 per cent. out of the total 100 per cent. It is noteworthy that traces of tyrosine were always found, although this amino acid hitherto has been considered absent from the gelatin molecule. The following amino acids were not present in gelatin: Valine, isoleucine, aminobutyric acid, and hydroxyglutamic acid. Unidentified sulphur derivatives were also found in the gelatin molecule. Since isoleucine and valine are absent, and considerable leucine is present, gelatin is a very suitable material for the preparation of optically pure leucine.

J. S. H.

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